BEHAVIOR THEORY
AND CONDITIONING

BY KENNETH W. SPENCE

Preface

A PSYCHOLOGIST confronted with the task of presenting a series of lectures to an audience composed of laymen, scientists from other fields, and psychologists of all varieties of special interests and viewpoints is in a peculiarly difficult position. As far as the lay portion of his audience is concerned, the difficulty arises primarily from the expectation that the discussion will be concerned in some degree at least with the familiar problems of everyday life. Somehow the layman is not quite prepared for the artificiality of the psychological laboratory even though he may have a good appreciation of the necessity for such conditions in the physical sciences. As a consequence the phenomena presented by the experimental psychologist are likely to seem strange and, by virtue of their artificiality, rather meaningless and without significance to him.

In the case of scientists from other fields the difficulties stem from an expectation of a different sort, namely that of finding the kind of knowledge, general laws, and comprehensive theories that one typically associates with science. Again there is likely to be considerable disappointment, for psychology is at present very much lacking in such integrating devices. Indeed, it is as yet in a very primitive state of development, one in which the primary concern is the formulation of empirical laws encompassing fairly circumscribed areas of behavior. While historians of science could undoubtedly find important differences, psychology today is in many respects at about the point that physics was at the time of Kepler and Galileo. Whatever the deficiences of this analogy such a perspective may at least greatly help the physical scientist to appreciate the current methodological problems of psychology.

Finally, even the psychologists in the audience present somewhat of a problem. The undeveloped nature of psychology as a science with its lack of integrating theoretical conceptions naturally makes for a wide variety of different and quite unrelated

areas of study. Instead of psychology then, we really have a large number of different psychologies or areas of psychological study between which there are as yet very few if any connections. Unfortunately the rather fragmentary attempts at theoretical integration that have been made, each with its own terminology, have as yet only served to accentuate this diversity, creating even more different kinds of psychologies and different kinds of psychologists.

Despairing of being able to meet this diversity of interests, backgrounds, and expectations, it seemed best for the purpose of these lectures not to attempt a comprehensive treatment of the general field of modern-day psychology but instead to confine the presentation to certain problems, theoretical and empirical, related to the research areas in which I have been interested during the past 20 years. Essentially this research and its accompanying theory represent an extension of the pioneering work in the area of the conditioned reflex of I. Pavlov in Russia and of Clark L. Hull in this country. However, in view of the fact that this was the first occasion that psychology had been represented in this lecture series, it seemed appropriate to devote the opening lecture to a brief history of the development of psychology as a science. The particular account given is heavily weighted in the direction of experimental psychology and in no way represents an adequate treatment of the development of the many areas of present-day psychology. Instead, major emphasis was given to some of the currently important methodological problems of psychology.

The five following lectures present empirical data from a number of simple conditioning situations and a theoretical structure or schema that has as its primary purpose the integration (derivation) of these data. The seventh and final lecture attempts to show how the theory, developed on the basis of these simple conditioning experiments, may be extended to more complex types of behavior phenomena such as selective and paired-associate learning. In addition to these empirical and theoretical objectives emphasis has been placed on the discussion of some of the problems and issues that arise in connection with such theoretical formulations.

The chapters as presented in this volume follow closely the

delivered lectures. The necessity for keeping each lecture to approximately one hour required a selection and arrangement of material that I would not have otherwise followed. Also because of this requirement some relevant material had to be omitted; in such instances selection has been on the basis of supporting the best theoretical guesses that I could make at the time. I have no doubt but that subsequent events are likely to prove embarrassing at many points. It would, indeed, be surprising at this stage of our knowledge if this were not the case.

The lectures have benefited greatly from the critical reading of parts of the manuscript by some of my former students, I. E. Farber, H. H. Kendler, and Janet A. Taylor, to whom I wish to express my gratitude. Larry Stein, my research assistant, contributed in a number of different ways, preparing and checking the references, making many of the calculations, and fitting the curves to the data in Chapters 4 and 7. Mr. Stein also worked out, following a suggestion of Frank Restle of the Human Resources Research Office, George Washington University, the method described in Appendix A for determining the expected probabilities of occurrence of two mutually incompatible responses. I also wish to acknowledge the draftsmanship of Leonard Ross and the typing of the manuscript by my secretary, Moyra Calder. I am greatly indebted to the many students of whose theses I have made extensive use.

Finally I should like to convey my sincere thanks to Dean Sinnott and the members of the Silliman Committee of Yale University for the invitation to deliver the Silliman Lectures. Many psychologists have expressed to me their pleasure at this recognition of our field, and I feel highly honored to have been asked to represent psychology in this distinguished series of scientific lectures.

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1. Historical and Modern Conceptions of Psychology

WRITING in a textbook in 1892, William James expressed himself as follows concerning the status of psychology as a natural science: "A string of raw facts; a little gossip and wrangle about opinions; a little classification and generalization on the mere descriptive level; a strong prejudice that we *have* states of mind, and that *our* brain conditions them: but not a single law in the sense in which physics shows us laws, not a single proposition from which any consequence can causally be deduced. We don't even know the terms between which the elementary laws would obtain if we had them. This is no science, it is only the hope of a science" (1892, p. 468). One suspects that the more optimistic psychologists of the period shrugged off this none too complimentary evaluation of their field with a variety of excuses such as, for example, that psychology was, as an experimental endeavor, still very much in its infancy. Indeed, if one measured from the time of founding of the first psychological laboratories in Germany during the seventies, psychology was less than 25 years old. Or, possibly, the defense was based on an appeal to the complexity of the phenomena psychologists sought to investigate and to their relative inaccessibility to observation.

At the time, the claim that not much could be expected of as young a science as psychology was a reasonable one. After all, there must be a period of purely empirical fact finding in order to provide a sufficiently comprehensive set of low order laws before the more abstract, integrative aspects of science can be expected to appear. A quarter of a century was, perhaps, too short a period to develop much in the way of a widely accepted systematic body of generalizations of any scope.

But what about now, 1956, some 60-odd years later and after almost a century of experimental study of psychological phenomena? Does psychology qualify as a natural science today?

While it is probably safe to say that some areas of psychology have attained a respectable level of scientific development, it would nevertheless have to be admitted that much of the field does not qualify. In this connection it is pertinent to note that psychology as such was not included among the fields of natural sciences designated in the Act of Congress establishing the National Science Foundation, although an area of research referred to as general experimental psychology has been included in the division of biological sciences. If we may accept this somewhat dubious criterion of the attainment of sciencehood, then approximately five per cent of current psychologists would qualify as natural scientists, this being the percentage of the total membership of the American Psychological Association that belongs to the Division of Experimental Psychology.

That the progress of psychology toward the goal of being a natural science has been slow and, even to some psychologists, disappointing is readily apparent. It is reflected, for example, in a comment that E. G. Boring of Harvard University made in the opening paragraph of his book, *The Physical Dimensions of Consciousness*. After calling attention to the fact that Descartes's dichotomy between body and mind occurred just at the time that science was beginning the development that was to make it the dominating influence in modern civilization, Boring wrote as follows:

> We all know how successful the physical sciences have been and we can also see that biology has prospered in abandoning a vitalism and identifying itself with the physical side of the Cartesian dichotomy. If Descartes was right, if there are these two worlds, then the success of science in attacking the one forms a challenge for the creation of a science of the other. . . Yet, if psychology is coordinate with physics and if the scientific method is applicable to both, then it seems strange that psychology has come such a little way when physics has ramified into many fields and has come so far (1933, p. 3).

While not a historian or methodologist and hence not in a position to base my beliefs on careful comparative study of the developments in psychology during its first hundred years with

those in other sciences during a comparable period, I nevertheless share this view that the development of psychology as a natural science has been much slower than might have been expected. Especially does this seem to me to be the case when we consider some of the advantages that a science beginning in the late nineteenth century would have as compared with physics, which, as an experimental science, began some 300 years earlier.

The early physicists, Galileo and his contemporaries, not only had to evolve a new conception of scientific knowledge and a radically new approach to the attainment of this knowledge but also had to face open, even hostile, opposition in the pursuance of this task. Rejection of the dominant Neoplatonic and scholastic conceptions that the observed facts must be deducible from and hence must conform to some existent authoritarian and rational synthesis met the most violent opposition, not only from Church authorities but also from contemporary university professors. Quite in contrast, by the time psychology set out to be an independent experimental discipline the success of the new empirical approach had been so great that such opposition had long since disappeared. Moreover, the success during the intervening period of the application of the methods of physical science in the field of biology helped to create an atmosphere quite favorable to the attempt to extend their use to the study of psychological phenomena.

In addition to starting out in an atmosphere highly supportive of its endeavors, psychology also had the benefit of all that had been learned about the methodology of science in 300 years. Thus it had available many examples of the problems faced by scientists at various stages of development of their field and the manner in which they were approached and solved. The role of measurement and experimental control, the place of concepts and laws, the functions that theories play in the ordering of knowledge had all been revealed many times over. Many of these scientific tools, particularly techniques of measurements and experimentation, were directly applicable to the problems of psychology and could be and were taken over and used. Moreover, the psychologist has recently had the advantage of the analysis of these methods by the philosophers of science whose writings have helped greatly to clarify not only

the relations among the different kinds of concepts employed in science but also the relation of the language of science to its empirical basis—the events in the consciousness or sense experience of the individual scientist.

Undoubtedly there are other advantages that psychology has had as a consequence of its late start, not the least of which would be the relatively greater support, financial and otherwise, that all scientific endeavors have received from our society in recent years. But enough has been cited, I believe, to indicate the basis of the great expectations that one might legitimately have held and the consequent disappointment that many have felt in the progress of psychology as a natural science.

As to the factors that have held back psychology during the first century of its existence I should prefer to confine my discussion primarily to those operating in the earlier portion of the period. It would take a person of far more diplomatic skill than I possess to dare go into all of the contemporary obstacles. In turning to the consideration of some of the earlier factors that may account for the slow development of psychology as a natural science, the discussion will be presented in the form of a historical account of what the first self-acknowledged psychologists conceived their task to be and how they proceeded to try to accomplish it. This account will also provide a general historical perspective for the research to be described in the later chapters.

CLASSICAL PSYCHOLOGY

Scientific experimental psychology as an independent discipline had its beginning in the work of men whose original training was in the biological sciences, particularly physiology. Thus the immediate forerunners were such well-known physiologists as Johannes Mueller, Fechner, and Helmholtz, while the first acknowledged experimental psychologist, Wilhelm Wundt, obtained a medical degree and taught physiology at Heidelberg for some years prior to turning, in 1870, more specifically to what he regarded as the study of psychological phenomena. While the interests of Wundt and his co-workers were, in part, concerned with the mediating neural processes underlying psy-

chological or mental phenomena, particularly the sensory mechanisms, they considered that a portion of their work called for a uniquely new and independent science, psychology. This aspect of their work, which had to do with the mind or consciousness per se, was dominated by ideas and concepts that had their origins in the writings of such philosophers as Descartes and the early British empiricists, Locke, Berkeley, and Hume, and the later ideas of the British associationists. As Boring (1933) has pointed out the establishment of the dichotomy between mind and body by Descartes provided the raison d'être of an independent science of psychology. Descartes introduced in his method of contemplative "meditation" the basic method of studying the mind, and he himself employed this earliest form of introspection to study the higher forms of cognition and the more complex emotional states. Interest shifted in the subsequent writings of the English empiricists from these central states to the peripherally aroused, simpler mental states known as sensations.

Between the psychological writings of these early philosophers and the beginning of experimental psychology a period of approximately 150 years elapsed, during which there was great interest in the mind and considerable writing and speculation concerning its nature. It was in this period that the conception subsequently known as classical psychology arose. Stemming from the writings of the English empiricists, classical psychology had its beginning in the speculative writings on the human mind of the English physician-philosopher Hartley and the Scottish philosopher Thomas Brown. Its pre-experimental culmination is to be found in the sensationistic-associationistic psychologies of the British associationists, James Mill, his son John Stuart Mill, and Alexander Bain.

Wundt's new experimental psychology was essentially one with this early speculative classical psychology. The main differences were that more precise experimental procedures were introduced and considerable attention was given to the laws connecting the mental phenomena with the environment on the one hand and with their physiological correlates on the other. As such the work of Wundt and his successors in the German laboratories essentially constituted the final, experimental phase

of classical psychology. This period came to an end in the first decade of the present century with the rise of functional psychology and behaviorism.

Classical psychology, whether pursued in the armchair of the empiricist-philosopher or in the laboratory of the new experimental psychologist, was conceived as the study of mind or consciousness as revealed through self-observation or introspection. Influenced, undoubtedly, by the atomistic physics of the period and the success of analysis in chemistry, it conceived its problem to be that of analyzing all possible states of consciousness into elements. These elements were regarded as not further decomposable and as providing the materials from which all mental states were compounded. Using himself or another person as a subject, the classical experimental psychologist presented various kinds of stimulation under controlled conditions that included a set to observe and report inner experiences. These situations might consist in the presentation of some visual or auditory stimulus to which the subject was to make some simple response, such as pressing a key in a reaction time experiment, or they might involve some complex problem which he was to solve. Primary interest was not in the overt response made by the subject although it was often recorded. Rather the concern was with the verbal account that the subject gave of the conscious experiences he had during the period of self-observation.

Under the set provided by the instructions these complex states of consciousness, perceptions, images, ideas, thoughts, emotions, and so on, were revealed to consist of such irreducible elements as sensations, feelings, and some kind of relational and thought elements. One of the major sources of disagreement among introspective psychologists during this period was the number of such irreducible elements. Some psychologists, apparently inspired by the chemists' success in finding new elements, sought to find new kinds of mental elements. Indeed a whole new school of psychology was established by Külpe at Würzburg on the claim of having discovered a new thought element. Other psychologists, particularly Titchener, the leading representative of classical psychology in this country, attempted to reduce the number to a single element, sensation.

In addition to this task of analyzing consciousness into its elements, there were two other stated, although somewhat neglected, objectives of classical psychology. One was to discover and formulate the principles of synthesis whereby the mental elements were compounded into the more complex states of consciousness. The main laws here were the so-called laws of association: contiguity in time, contiguity in space and similarity, etc. These laws, describing the conditions whereby conscious experiences become organized into spatial and temporal patterns, Wundt took over directly from the British associationists. Secondly, as was mentioned earlier, the experimental classical psychologists were also interested in discovering the laws relating the states of consciousness to the stimulating conditions (so-called outer psychophysics) and to the functioning of the nervous system (inner psychophysics). The latter laws were regarded as providing for the explanation of the conscious states, whereas the laws of association were supposed to be merely descriptive.

STRUCTURALISM AND PHENOMENALISM

In its later phases, classical experimental psychology became subdivided into two main branches, structuralism and phenomenalism. Structuralism represented a later development of the elementarism of Wundt. In its final stages, as represented by the work of Titchener in this country, structuralism belatedly came to the realization that the concept of sensation is a logical, systematic construct and not a raw datum. In its place the structuralist substituted as the end product of introspective analysis, first, the attributes of sensations and, subsequently, the so-called dimensions of consciousness: quality, intensity, extensity, and protensity. The final form of this reconstructed structuralism, as exemplified by Boring (1933), considered its main task to be that of understanding the physiology of these dimensions of consciousness.

The second branch, phenomenalism, or as it has also been called, phenomenology, had its origins in the work of Brentano, one of Wundt's chief rivals. Brentano rejected sensory contents as the materials of psychology and instead proposed that psy-

chical acts such as perceiving, judging, relating, recalling, and so forth were what was revealed by introspection. In his formulation the focus of attention shifted from the peripheral events back to the central regions of the mind. Subsequently the work of the Würzburg group on thinking and the Gestalt psychologists on perception brought out further objections to the type of analytical introspection employed by the Wundtians and the later structuralists.

The Würzburg psychologists were unable, under the particular set their observers employed, to reduce the experiences occurring during thinking to sensory materials as had the Wundtians. Indeed they found it difficult to establish descriptive categories in this area since our vocabulary is woefully inadequate to describe and communicate the contents of experience or processes occurring during such activities. The Gestalt psychologists also attacked the classical notion of perceptual experience as being compounded of sensations and criticized this conception as resulting from the suggestive influence of the knowledge from physiological psychology concerning the structure of the sensory mechanisms. The sensationistic interpretation of perception was suggested, they claimed, by the knowledge of the mosaic structure of the sensory surfaces and the afferent nerves with their bundles of fibers. The Gestaltists pleaded for an unbiased description of immediate experience, one that was free from any systematic preconceptions such as those held by the sensationists. Phenomenological introspection aimed to provide such an initial description of consciousness. It employed terms from everyday language, even slang words. While such pure description has its virtues, especially in a young science, sooner or later some kind of generalization of the initial descriptions must be attempted if the systematic kind of knowledge science seeks is to be obtained. Whereas structuralism tried and failed to formulate a system of constructs and laws of mental analysis and synthesis that gained general acceptance, phenomenalism never succeeded in providing any degree of integration of its observations.

The classical movement in psychology with its emphasis on the method of introspection came to an end with the functionalist-behaviorist revolution in America just after the start

of the present century. Looked at in the light of the subsequent conception of psychology as the science of the behavior of living organisms in which the laws sought after state under what environmental and organic conditions organisms behave in the manner they do, this early work of the purely introspective psychologist must be regarded as an unfortunate false start. This is not to say that the problems in which they were interested were of no importance. Rather, as it appears now, their work was less effective because they relied too heavily on an unreliable (i.e., intersubjectively inconsistent) method and because other simpler, although perhaps humanly less interesting, matters had to be investigated first. The laws that these psychologists sought, for the most part, fall into the category of what we shall later refer to as R-R or response-response laws. These R-R laws represent only one small segment of the total framework of a science of behavior, and unfortunately not a very basic one at that. One cannot help but speculate on how much further advanced psychology would be today as a science if the physiologists who started it had been less intrigued by the ideas of the classical speculative psychology of the philosophers and more concerned with the problem of discovering the laws relating mental phenomena, however introspectively reported, to the stimulating conditions and neurophysiological variables.

FUNCTIONALISM

Not only was a goodly portion of the first 30 years of work thus lost, still another 20 years of polemics were required before psychologists could concentrate their full energies on the task of building an objective science of behavior. The period 1900 to 1920 witnessed the rebellion against the classical orthodoxy. On the Continent the Würzburg group took the lead, questioning the adequacy of the classical type of introspection, while in this country the group of psychologists known as the functionalists was in the vanguard. If this were primarily a book about the history of psychology, which it is not, it would be appropriate at this point to trace the antecedents of functionalism to such influences as the biological theory of evolution, the writings of the early American psychologists, particularly

William James, and last but by no means least, the pragmatic temper of America with its interest in success and the practical application of knowledge as a means of assuring it.

Functional psychology, as a formal school, had its birth at the University of Chicago under the guidance of John Dewey and James Rowland Angell. One of the leaders of functionalism, indeed its most effective spokesman, Angell was extremely influential both in his capacity as director of the psychological laboratory at Chicago for 26 years and in his writings. It was in his laboratory that Watson, his student, began his animal studies and developed the ideas that led eventually to behaviorism. Behaviorism is itself, of course, a functional psychology but one that, as we shall see, went beyond the functionalism of Dewey and Angell.

Objecting to the classical psychologist's preoccupation with the structural composition of consciousness, that is, with the question of *what* consciousness *is*, Angell and his group proposed to shift the focus of attention to the problems of how consciousness operates and what uses or functions it serves. The point of view they adopted was a Darwinian one in which the different operations of consciousness, sense perception, imagination, and emotion, were all regarded as different instances of organic adaptation to the environment. Interest was thus directed to the overt behavior of the organism in relation to its environment as well as to the functioning of the mind. However, the functionalists did not propose to neglect consciousness, and they continued to regard introspection, the direct examination of one's own mental processes, as the fundamental method of psychology.

This new functional orientation led to a rapid expansion of the kinds of phenomena that psychologists sought to investigate. Being concerned with the problem of the adjustment of the organism they became interested in the adaptive capacities of the individual and methods of measuring them. Thus was set in motion the development of mental tests and their subsequent use in a variety of situations, particularly in education. Interest in adaptive functions also led to a stressing of research on the genetic and developmental aspects of behavior both in the individual and in the species. Studies of the behavior of children

were instituted, and laboratory experimentation with animals, which had just got started with Thorndike's researches at Columbia on cats and dogs in puzzle boxes, was taken up with great enthusiasm at Chicago. Watson established an animal laboratory there and began the series of studies of animal behavior that led him eventually to his behavioristic position.

While functionalism admitted behavior into psychology, consciousness was still its first love. To a considerable extent the behavioral data were considered to possess significance *only* insofar as they were able to throw light upon conscious processes. Thus in animal experiments, after making the behavioral observations, the practice was to infer the animals' consciousness and then show how these processes functioned in the behavioral adjustment to the environment. It was this insistence that behavior data must have analogical reference to consciousness that finally led Watson to rebel against functionalism and to demand that psychology change its viewpoint so as to accept the facts of behavior regardless of whether or not they have any bearings upon the problem of consciousness.

BEHAVIORISM

Watson came to his behavioristic position primarily from his interest in animal research. There he had found that he could pursue quite successfully the study of behavior, even the traditional problems of sensory discrimination, without having to bring in conscious material. He simply proposed to extend the same objective behavioral methods of observation to the study of human behavior.

Watson's declaration of independence from the consciousness-dominated psychology of the period provoked a stormy period of polemical activity which, as it continued, was marked by more and more extreme statements on both sides. If one goes back to Watson's initial formulation, the 1913 *Psychological Review* article entitled "Psychology as the Behaviorist Views It," it may be seen that he did not deny the existence of consciousness, as has sometimes been represented, but simply proposed to use it in the same way that other scientists do. His statement is so clear I should like to quote portions of it:

> Psychology, as the behaviorist views it, is a purely objective, experimental branch of natural science which needs introspection as little as do the sciences of chemistry and physics . . . It can dispense with consciousness in a psychological sense. The separate observation of "states of consciousness" is, on this assumption, no more a part of the task of the psychologist than of the physicist. We might call this the return to a non-reflective and naive use of consciousness. In this sense consciousness may be said to be the instrument or tool with which all scientists work (1913, p. 176).

In other words Watson was arguing that, contrary to the belief of the introspective psychologists, psychology does not, or at least should not, have a unique subject matter, mental or conscious events as contrasted with matter or physical events. Rather, he insisted, the data of the psychologist are of exactly the same kind as those of the physical scientist. That is to say, immediate experience, the initial matrix out of which all sciences develop, is no longer to be conceived as the special province of psychology. The psychologist, like other natural scientists, simply must take consciousness for granted and proceed to his task of describing certain happenings occurring in it and discovering and formulating the nature of the relationships holding among them. The subject matter of psychology is exactly the same *in kind* then as all other sciences; any differentiation among the sciences is merely a matter of convenience, a division of scientific labor resorted to as the amount of detailed knowledge increases beyond the capacity of a single person's grasp.

We shall not attempt to follow the subsequent developments within behaviorism to the present-day forms of objective psychology except to discuss a little further the problem of what in the observable experience of the scientist is to be included as data in psychology. Watson, as we have seen, proposed to include only the observable behavior of living organisms other than the observing individual. He refused to admit the sensations, perceptions, and similar phenomena of the introspective psychologist. In his first article expounding this new behavioristic viewpoint Watson was quite frank in admitting that he was

being somewhat arbitrary in rejecting these phenomena. One of the main reasons he gave for doing so was that he was not very optimistic concerning the possibility of any agreement being reached concerning these introspective data among psychologists of different training. Furthermore, he was more interested in getting on with the job of studying behavior.

LOGICAL EMPIRICISM (OPERATIONISM)

In later articles Watson attempted to offer other grounds for the rejection of consciousness, as did subsequent defenders of behaviorism. Typically these latter arguments have led into philosophical questions, the answers to which had best be left to the philosophers. From the point of view of the scientist the most important criterion of what aspects of the initial data of observation are to be included is the pragmatic one of which can be reliably made a matter of public record and thus taken out of the realm of private experience. For science is concerned only with such public knowledge.

Recognized today as being needed for this purpose is a language or vocabulary, some of the terms of which may be related in a highly consistent fashion to the direct experiences of the scientist. How these terms and the abstract concepts defined from them are to be introduced so that they are meaningful, precise, and of sufficient scope to provide for explanatory and predictive principles in the form of general laws and theories is an extremely important problem. Fortunately, considerable attention has been given to this problem in recent years, particularly by a group of philosopher-logicians known as logical empiricists. Accepting the requirement that all terms employed by the scientist, no matter how abstract, must ultimately be referable back to some primitive or basic set of terms that have direct experiential reference, these writers have suggested that such a basic vocabulary for science is provided by the terms designating directly observable physical objects such as tables, chairs, and cats, their properties, e.g., green, hard, and loud, and the relations between them such as between, before, and below. Different members within the group refer to this vocabulary of basic terms differently, calling it variously the "ob-

servation vocabulary," the "observable-thing language," and so on.[1]

Forming as it does the verification basis of all the scientist's statements, it is essential that this class of basic observables display a high degree of intersubjective agreement among different observers and in the same observer from one occasion to another. As is well known, physicists have depended to a great extent on a subclass of such observations that possess this characteristic in the highest degree, namely, pointer readings involving the observation of the coincidence of points in space or time. Indeed, as historians of science have pointed out, the great advances made in physics at the time of Galileo were largely due to his decision to limit or reduce the extent of eligible experiences that he would admit for study to the small fraction represented by these pointer readings.

For the most part modern objective psychologists have adopted this common-sense approach and have confined their observations to items that can be represented in such a basic scientific language. This does not mean, however, that it is no longer possible for them to deal with the types of problems that interested the classical psychologists. Thus my colleague Gustav Bergmann (1954) has even shown how the classical problem of mental analysis may be treated behavioristically in a manner that gets rid of all the pseudo problems that plagued the introspectionists and brings out whatever genuinely scientific ones they had. Involved in this translation are, roughly speaking, correlations among verbal responses of the subject or, as was indicated earlier, R-R laws.

Similarly, the behavior scientist has made extensive use of verbal responses in the formulation of hypotheses concerning the nature of higher mental processes. To some critics of the

1. Actually, the first to acquaint the scientist in a nontechnical manner with the requirements of operational definition of their concepts was the physicist Bridgman (1927). The subsequent technical developments have been mainly the work of the logical empiricists, the statements of it by Carnap (1936, 1937), Bergmann (1943, 1954), and Hempel (1952) probably being among the most important. Attention was first called to the importance of operational analysis in psychology by S. S. Stevens (1939), followed by such technical papers as that of Bergmann and Spence (1941) and the more recent comprehensive analysis of the logical structure of psychology by Bergmann (1953).

modern behavioristic viewpoint this acceptance of verbal reports as part of the data has seemed to represent an abandonment of the strict behavioristic position. Such a contention, however, fails to note a very important difference between the behavioristic treatment of such data and their use by the introspectionist. The introspectionist, it will be recalled, assumed a strict one-to-one relation between verbal responses of his subjects and the inner mental events. Accordingly, he accepted these introspective reports as *facts* or *data* about the inner mental states or processes which they represented. The behavior scientist takes a very different position. He accepts the verbal response as just one more form of observable behavior, and he proposes to use this type of data in exactly the same manner as he does other types of behavior variables. Thus he attempts to discover laws relating these verbal responses to environmental events of the past or present, and he seeks to find what relations they have among each other and to other nonverbal types of response. In contrast, then, to the introspectionist's conception of these verbal reports as mirroring directly inner mental events, the behaviorist uses them either as data in their own right to be related to other data or as a base from which to make inferences concerning hypothetical, covert psychological processes.

MODERN OBJECTIVE PSYCHOLOGY

I should like to turn now to an analysis of the general conception of their science that the majority of psychologists in this country hold today. While behavioristic in outlook, this conception is probably better described as objective psychology. As such it does not require adherence to the orthodox doctrines of Watson. Moreover, it provides within its framework not only for alternative theoretical formulations but also for a purely empirical approach such as that espoused by most of the psychologists interested primarily in applied problems. Focusing attention as it does on the behavior of the organism in relation to two other classes of events, the environmental surroundings and the organic conditions of the organism, the concepts or variables of this modern conception fall into three groups or classes: (1) Response (R) variables: qualitative descriptions

or measurements of the behavior properties of living organisms. (2) Stimulus (S) variables: qualitative descriptions or measurements of events or properties of the physical and social environment in which the organism behaves. (3) Organic (O) variables: qualitative descriptions or measurements of the anatomical and physiological properties of organisms.

Like every other scientist, the psychologist is interested in discovering and formulating the relations or laws holding among these different classes of variables. The several types of laws with which psychologists have been concerned are as follows: (I) $R = f(R)$; (II) $R = f(S)$; (III) $O = f(S)$; (IV) $R = f(O)$.

The first class, $R = f(R)$ laws, describes relations between different attributes or properties of behavior; they tell us which behavior traits are associated. This type of law is investigated extensively in the fields of intelligence and personality testing, and the laws that have been discovered have formed the basis of much of our technology in the areas of guidance, counseling, and clinical diagnosis. These empirical R-R relations also form the starting point for the theoretical constructs of the factor analysts. Beginning with the intercorrelations among a large number of test (response) scores these theorists have attempted by means of their mathematical methods to discover a minimum set of hypothetical factors that could account for the variance in the behavioral measures. The so-called field theory of Lewin (1935) was also concerned primarily with this R-R type of law, his theoretical concepts being introduced in terms of response variables and eventually returning to other response variables.

The second class of laws, $R = f(S)$, relates response measures as the dependent variable to the determining environmental conditions. There are really two subclasses of laws here, one relating to the environmental events of the present and the second to events of the past. The first subclass includes the traditional laws of psychophysics, perception, reaction time, and emotions. These laws describe how behavior varies with changes in the present physical stimulus. The theories that have been investigated by this kind of law are primarily of the reductionistic type, involving hypotheses as to the nature of the underlying neurophysiological mediating mechanisms.

Insofar as the behavior at any moment is a function of environmental events that occurred prior to the time of observation one is dealing with laws of the second subclass. The most familiar instance of this kind of relation is represented by the so-called learning curve which relates the response variable to previous environmental events of a specified character. Laws of primary and secondary motivation are other examples that fall in this group. These laws have provided the starting point of most theories of learning, although some learning theories have been initiated on the basis of neurophysiological laws.

A survey of the research literature since the beginning of experimental psychology will reveal the extraordinary extent to which the interests of psychologists have turned more and more to the investigation of laws of the first and second class. Less and less interest has been shown in the third and fourth class of relations which involve, as one of the members, physiological and anatomical variables. This trend was apparent even in the early developments in psychophysics. Whereas classical psychophysics was concerned primarily with the mediation problem of tracing the chain of physiological events beginning with the sense organ activity, the later psychophysicists shifted their interest to the correlations between the end terms, the stimulus event and the sensation, or sensory response. With the advent of the functional and behavioristic viewpoints interest in physiological research declined markedly until it now represents a relatively small proportion of the total research activity of psychologists. Presumably as psychology attains a fairly well-developed body of environmento-behavioral laws more and more attention will once again be directed to these physiological variables. In the meantime a number of psychologists, particularly the Gestaltists and those interested in simple sensory phenomena, have continued to investigate the class III type of laws. Mention should also be made of the continuing interest in the central neural mechanisms and their relations to various kinds of behavioral phenomena, including learning and motivation, that Lashley and others have maintained. These investigations have provided us with class IV laws.

The discovery and formulation of these various types of laws in psychology have required, just as in other sciences, three

major methodological developments: (1) the specification of operationally defined, quantitative concepts that permit the relations among the variables to be expressed in terms of mathematical functions; (2) the development of instruments and experimental designs that provide for the isolation, control, and systematic variation of the factors in the situation under observation; and (3) the introduction of theories.

The role of the psychologist's theories in this task requires some clarification. The term "theory" as used in an advanced science such as physics typically refers to a system of highly abstract concepts and principles that serves to unify or organize into a single deductive system sets of laws that previously were quite unrelated. The classical example is the Newtonian integration of the previously unconnected laws of planetary motion and mechanics. Other well-known instances are Maxwell's electromagnetic theory of radiation and the kinetic theory of gases. In psychology, on the other hand, the term theory more frequently refers to something quite dissimilar, at least something that serves a very different function. Primarily because of the great complexity of psychological phenomena, the psychologist has often been unable to isolate, experimentally, simple systems of observation in which all of the relevant variables were known to him and under his control. Moreover, even in instances in which this was possible, the determining conditions have usually been so many and so complex in their interrelations that it is extremely difficult to arrive at any comprehensive law or set of laws. In these circumstances the psychologist has introduced what he has called a theory. Essentially such theorizing consists in hazarding hypotheses as to what the unknown factors might be in terms of their possible relations to the known variables and guesses as to the structure of the laws relating the already known variables on the basis of the existing data. In other words, whereas the term theory in modern physics refers to a system of constructs that serves to interrelate sets of already established laws, in psychology the term typically is applied to a device employed to aid in the formulation of the empirical laws describing a realm of observable phenomena.

Most fields of psychology are still in the stage of attempting to identify fruitful concepts that will lead to the formulation

of the laws of the system under observation. However, a few areas would appear to have gone beyond this stage of searching for the relevant variables and have reached the point at which the majority of the variables have been specified and fairly precise determinations have been made of the laws relating them. In certain of these more advanced fields, particularly those concerned with some of the sensory functions (e.g., vision and audition), fairly comprehensive theories that attempt to integrate the existing empirical laws have been put forward. In other areas, intermediate in their development, the theories are more limited in scope, being concerned with relatively circumscribed sets of observations.

OVEREMPHASIS OF PRACTICAL AND REAL PROBLEMS

In concluding this survey of the past and present state of psychology as a science I should like to add one further historical comment and then some final remarks concerning the present scene. One of the intents of my historical survey has been to bring out the fact that psychology as an objective, natural science is really a very young field of study. Indeed, if we measure from the date of Watson's first break with the consciousness-dominated conceptions of psychology the period of its existence is just 40 years. And as far as research is concerned much of the first 10 to 15 years of this period was lost in defending the new point of view against attacks upon it by the old guard and attempting by purely polemical means to show the greater fruitfulness of the new approach. While Watson made a start in the areas of child research and introduced objective methods for studying sensory and learning processes, more and more of his time came to be taken up with pure sales talk.

The brevity of the period of research endeavor by this group, particularly in the important areas of motivation and learning, is probably no more strikingly revealed than by recalling that the laboratories of Hull and Tolman, the two most influential behavioristic psychologists, did not become active until about 1930, only 25 years ago. While considerable experimental work had been done in these fields, particularly in learning by the functionalists, it was not until the work of Hull and Tolman and

their students that serious attempts were made at theoretical integration of the data. Even these efforts have had to be highly programmatic in nature because of the lack of sufficiently detailed empirical knowledge.

This brings me to the present-day scene in psychology. Early in the chapter I stated that an analysis of the contemporary factors retarding the development of psychology as a natural science required a degree of diplomacy far beyond my limited skill in such matters. And yet I feel I would be remiss if I did not, at least, call attention to what appear to be at present two major deterrents to progress toward the establishment of a scientific body of psychological knowledge. The first of these retarding influences is reflected in certain attitudes that are exhibited in the area of psychological theorizing, namely the tendency to criticize theoretical concepts in this field as being too elementaristic, too mechanistic, and as failing to portray the real essence or true nature of man's behavior. In particular, these critics have complained about the artificiality of the objective types of concepts such as offered by the behavioristic psychologist. Thus they talk about such things as the impoverishment of the mind and object to what is described as a lack of warmth and glowing particulars in the behaviorist's account of psychological events.

To the writer such criticisms reflect essentially a lack of appreciation as to the difference between *scientific knowledge* of an event and other kinds of knowledge, e.g., the kinds of knowledge the novelist and poet portray. Either by reason of their training or because of their basically nonscientific interests these critics have apparently never really understood the *abstract* character of the scientific account of any phenomenon. The only reply that can be made to such a critic is to point out that the scientist's interests just happen to be different from his. There are, of course, other perfectly legitimate interpretations of nature and man than the scientific one, and each has its right to be pursued. The science-oriented psychologist merely asks that he be given the same opportunity to develop a scientific account of his phenomena that his colleagues in the physical and biological fields have had. If there are aspects of human behavior for which such an account cannot ever be developed,

there are not, so far as I know, any means of finding this out without a try. Unfortunately, the attitudes of too many psychologists with regard to this matter are not such as are likely to lead them to the discovery of such knowledge. The difficulty, in part, is that too many individuals whose interests are not those of the scientist have become psychologists. If these persons were aware of their different interests and were appreciative of what the behavior scientist is attempting to do, the kinds of knowledge he is attempting to build, much needless controversy would be eliminated.

The second factor in the current scene that I believe is somewhat unfortunate is the tendency of the great majority of persons working in the field to evaluate the significance of psychological concepts and research in terms of the degree to which they are applicable to some immediate practical or technological problem rather than the extent to which they enter into or contribute toward the development of a body of lawful relations of whatever degree of abstractness.

It is easy, of course, to understand why this predilection exists. The psychologist is under very pressing demands to help solve serious and important technological problems, particularly in the case of behavior disorders with all of the human suffering they cause. But, unfortunately, psychological phenomena are extremely complex. The behavior of living organisms, especially that of man, involves such a great variety of determining conditions and exhibits such a multiplicity of forms as to defy easy analysis and hence the discovery of lawful relations. As a consequence we find the psychologist being asked to cope with problems of immediate practical import, such as mental illness and juvenile delinquency, long before he has the adequate means, i.e., the laws, with which to do so.

The unfortunate situation in which the psychologist finds himself may perhaps be more vividly portrayed by the following imaginary happening. Suppose by some very strange set of circumstances that Galileo and his co-workers were somehow brought back to life in our modern world and were confronted with the wondrous array of machinery, electronic and otherwise, that exists today. Further, let us suppose that all the knowledge of the physical sciences that had been gained in the

meantime had somehow been lost and that there were no engineers or technicians around to appeal to for aid. Consider now the plight Galileo and his colleagues would be in if, as physicists, they were called upon to help keep these devices in good working order. It is not difficult to imagine that the degree of chaos and disorder would soon outstrip even that currently exhibited by human machines and that repair shops or institutes for the mechanically defective would in a very short time be overflowing with devices exhibiting serious forms of maladjustment.

In this predicament two courses of action would be open to our primitive physicists. They could, on the one hand, concentrate on the practical problems and attempt by trial-and-error methods to work out superficially understood recipes or rules of thumb for manipulating the various parts of the gadgets in an effort to keep them performing after a fashion. Or they could direct their main energies to the task of discovering the basic laws governing the behavior of the devices. But the latter undoubtedly would require a turning away from these complex devices to the investigation of simpler phenomena that would appear to have little or no obvious connection with the more complex. It is not difficult for the psychologist with similar interests to imagine the attitudes that the reincarnated Galileo would elicit in society and in his more repair-conscious colleagues if he were to insist on turning his back on the complex machines and start measuring the time it took little balls to roll down an inclined plane. In place of the scoffings of the scholastics who, because of a frame of reference that evaluated everything in relation to some grand rational synthesis, couldn't understand the significance of the empirical laws of motion, Galileo would find himself confronted with the criticism that his activities had little or no significance for the *really* important problems of life, such as, for example, keeping the television and radio sets in good working order.

While fully recognizing that my imaginary example is probably overdrawn and that it could be the source of some quite erroneous implications, I nevertheless believe that it does point up an unfortunate overemphasis in psychology today. This is that far too many psychologists, under the pressures of the immediate practical problems and also, I fear, driven by an

overzealous and self-conscious sense of professionalism, have adopted as the criterion of what is significant in their endeavors only that which gives the appearance of being immediately useful rather than the scientific criterion of whether the ideas or activities lead to the discovery of laws and the formulation of systematic theories of a comprehensive nature. Or, to put it another way, too much of the time and energy of too many psychologists is currently being spent on engineering rather than basic, scientific problems. Moreover, this is being done in spite of the fact that the history of other sciences has repeatedly shown that the range and adequacy of any engineering program rests upon the degree of development of a basic system of laws.

In concluding the discussion of this topic, I should like to be especially careful to make clear that I do not wish to imply that significant research in the scientific sense cannot be carried out in applied situations, e.g., in the factory or in the classroom. If the investigation is properly oriented many instances of the lower order types of laws, at least, can be formulated on the basis of research conducted under such conditions. My aim, rather, has been to emphasize the point that in the instances we have from other sciences of successful development of comprehensive systems of knowledge analysis and investigation of simple situations under artificial, controlled conditions quite remote from reality have played a decisive role. Thus the physicists' laws of dynamics do not deal directly with real bodies behaving in the real world but with such abstractions as mass points, frictionless rolling balls, a perfect vacuum, and so on. It is my personal belief that if psychologists are ever to attain a comparable level of synthesis of behavior phenomena they too will have to formulate such abstract concepts and laws. From my admittedly biased point of view such an achievement will require the extensive use of artificial, nonlifelike conditions of experimentation.

Finally, I should like to quote from a recent symposium on "Scientific Research and National Security" in which Allan Waterman, Director of the National Science Foundation, made the following very significant statement concerning medical research in this country: "The technological progress that

has occurred since World War II and the problems and progress that arise in medical science have demonstrated that a greater effort in basic research is needed in order to support the advances that are being made in applied and developmental fields" (1954, p. 215). One frequently hears similar pleas even in the case of the physical scientists, some of whom fear that the emphasis on applied research and development that is so characteristically American will soon leave us with a deficiency of basic theoretical knowledge. That these fears are not entirely groundless was or should have been brought vividly home to us by the extent to which our lead in the field of nuclear weapons is due to the work of theoretical physicists whose training and point of view toward research was gained in Europe rather than in this country.

But if there is such a pressing need for basic research in the physical and biomedical fields, how much more is this the case in psychology. Somehow psychologists and those interested in encouraging research in this field of knowledge must be made to see that the need for basic research that will lead to the discovery of general principles is even greater in this young, as yet undeveloped area than in the physical and biological sciences.

2. Selective Learning and Conditioning

IN THE FIRST CHAPTER we discussed, among other things, the complexity and interrelatedness of psychological phenomena, the attendant difficulties confronting psychologists in their attempts to isolate simple systems of observation, and the consequent necessity for extensive theorizing even in the early stages of development. One of the areas in which psychologists have been most persistent in their attempts to develop such simple situations and to construct theories in relation to them is the realm of behavior phenomena known as learning. The remaining chapters will be concerned with the efforts of psychologists to deal with some of the empirical and theoretical problems that have arisen in this realm of behavior.

Learning is a phenomenon with which everyone is more or less familiar. Indeed, each of us has been a subject in innumerable naturalistic experiments in which we ourselves were learners, and we have had frequent opportunities to observe this phenomenon occurring in others. While these everyday instances of learning have made us well acquainted with the fact that behavior often changes quite profoundly with successive experiences in the same situation, nevertheless, the information gained under these conditions has not been of a very systematic character. Neither nature nor, least of all, society provides the simplified conditions, let alone the necessary regularities among them, that are required for the discovery of a scientific body of laws about learning. The attainment of this kind of knowledge requires not only measurement but also control of the organism's behavior and systematic variation of the relevant environmental variables.

The fact that learning is such a pervasive process, one that ramifies into almost every aspect of behavior, has led many different kinds of psychologists to be interested in the phenome-

non. As a result we have a number of different approaches and kinds of contribution to the study of learning. The clinical psychologist, for example, has been very much interested in the conditions underlying the acquisition of neurotic and psychotic symptoms, while the child and social psychologists have concentrated considerable attention on the factors governing the learning of our social needs, customs, and attitudes. Similarly, the educational and applied psychologists have attempted to discover the kinds of environmental arrangements that provide the most efficient ways of acquiring symbolic and perceptual-motor habits and the various kinds of special skills that are employed in art, music, and industry.

For the most part the concern of these psychologists has been the practical one of discovering the specific, historical factors underlying the various characteristics of the behavior in which they are interested. The practicing clinician, for instance, needs to be able to identify the conditions or happenings in the life of an individual patient that were responsible for the particular personality or behavior syndrome exhibited. The interests of the theoretical-experimental psychologist are quite different. His primary concern is the formulation of general laws and theories that will have a wide range of application to particular instances of learned behavior. Thus, the learning psychologist, unlike the clinician, is not greatly interested in knowing and cataloguing all of the different and particular kinds of things in the social environment that serve, for instance, as goals or rewarding events. Rather, his interest lies in formulating laws descriptive of the relations holding between response changes on the one hand and the reinforcing property of such goal events on the other. Furthermore, the learning psychologist attempts to develop theories, either physiological or mathematical, that will explain these events in the sense of providing for their logical derivation and integration.

ANALYSIS OF SIMPLE TRIAL-AND-ERROR (SELECTIVE) LEARNING

As in the case of other sciences, in order to achieve his objective the learning psychologist has had to arrange for more

controlled conditions of observation than occur in everyday-life situations. To this end he has designed and made use of a variety of experimental situations that permit the isolation and manipulation of different combinations of environmental variables. A number of different kinds of laboratory learning situations have been employed by experimental psychologists including such types as verbal rote learning, maze learning, discrimination learning, complex problem solving, and multiple choice learning. However, in recent years three somewhat simpler types of learning experiments have been favored. These three are selective learning, instrumental conditioning, and classical conditioning.

In the present chapter the main features of these three latter types of learning experiments, particularly their stimulus conditions and the nature of the responses observed in them, will be analyzed. As we shall see, the primary purpose of these experimental arrangements has been to isolate simple phenomena of behavior change with a view to discovering the basic laws governing them.

We shall begin with the kind of learning problem known as *simple trial-and-error* or *selective learning*. This type of learning situation was one of the earliest used by psychologists, being introduced in an informal fashion by the English comparative psychologist, C. Lloyd Morgan (1896). It was employed for the first time in the laboratory by Thorndike at Columbia University (1898). In Thorndike's experiments the subjects (cats, dogs, or monkeys) were usually placed inside a box from which they were required to learn to escape by such means as pulling a looped string, turning a button, or lifting a latch. While the behavior of the subjects was often recorded in detail, the main indicator of the progress of learning was the decreasing time taken on successive trial occasions to make the response that led to escape from the box and the attainment of food.

Instead of discussing this problem-box type of selective learning, however, we shall for the purpose of our analysis employ the single choice point type of maze shown in Figure 1. Represented in the figure is the ground plan of the different parts of the apparatus. Placed in the starting box (SB), the

subject, usually a white rat, must learn upon reaching the choice point to enter and run down alley S_C to reach the goal object (a piece of food) in end box C and not to enter alleys S_A or S_B which lead to empty end boxes A and B.
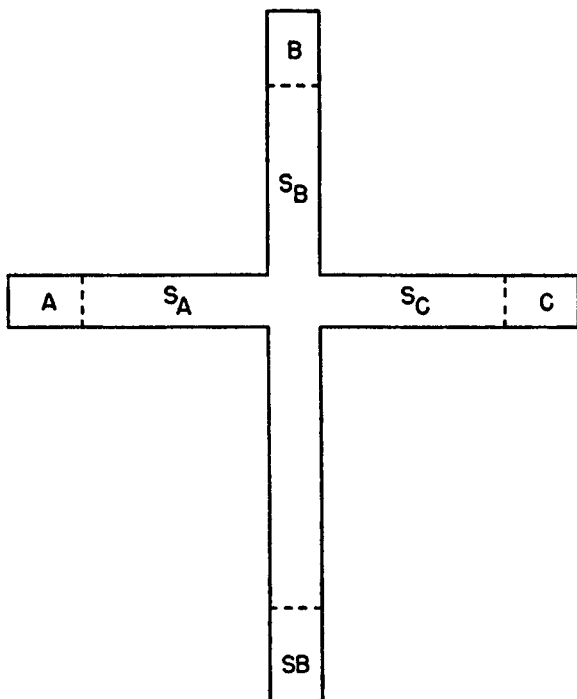
SINGLE CHOICE POINT MAZE

Fig. 1. Ground plan of the single choice point maze. SB = start box; S_C = path leading to loaded end box, C; S_A and S_B = paths leading to empty end boxes, A and B.

Four main components of this selective type of experimental arrangement may be distinguished: (1) a motivated organism; (2) the environmental or choice situation which affords a number of alternative response possibilities; (3) the behavior repertoire of the organism, i.e., the different responses that may occur in the environmental situation; and (4) the situation or

stimulus events consequent to the several responses. I shall discuss each of these briefly in the order they were mentioned.

Motivated Organism. Before instituting the experiment, the investigator arranges to have a motivated subject. For the present it is sufficient to state that the motivational condition of the subject may involve some primary appetitional or primary aversive need, produced by manipulation of the organism's environment in the immediate past or present. Examples are hunger resulting from the withholding of food for a period of time prior to the experiment or a state of pain produced by administering electric shock in the apparatus. Or, the motivational condition may involve some higher order or acquired drive that is under the control of the experimenter. Thus the subject may be made fearful by having a stimulus present that has previously been associated with shock.

Environmental (Choice) Situation. The second component of selective learning, the environmental or choice situation, typically offers a number of discriminable aspects, objects, or manipulanda that may serve as potential stimuli to which the subject may respond in a variety of ways. In the present apparatus these include not only the three alternative pathways but also such things as the wire top of the apparatus, the closed door leading from the starting box, various corners of the choice chamber, and so on. Attention should be called to the fact that the environmental situation does not provide or contain a reinforcing event such as food or escape from shock. Nor is the relation of its various aspects or parts to any reinforcing event immediately apparent or perceptible without the appropriate response occurring. In this respect this type of situation differs radically from the *umweg* type of problem situation favored by the Gestalt psychologists. In the latter the incentive or reinforcing object is present in the situation, and its relation to some particular part is perceptible. The two situations are thus seen to be very different, and they undoubtedly involve emphasis upon different psychological processes.

Responses. The combination of the motivating state and the environmental situation impels the subject to respond and to continue responding to various aspects of the situation until a reinforcer is obtained or until removed from the situation. In
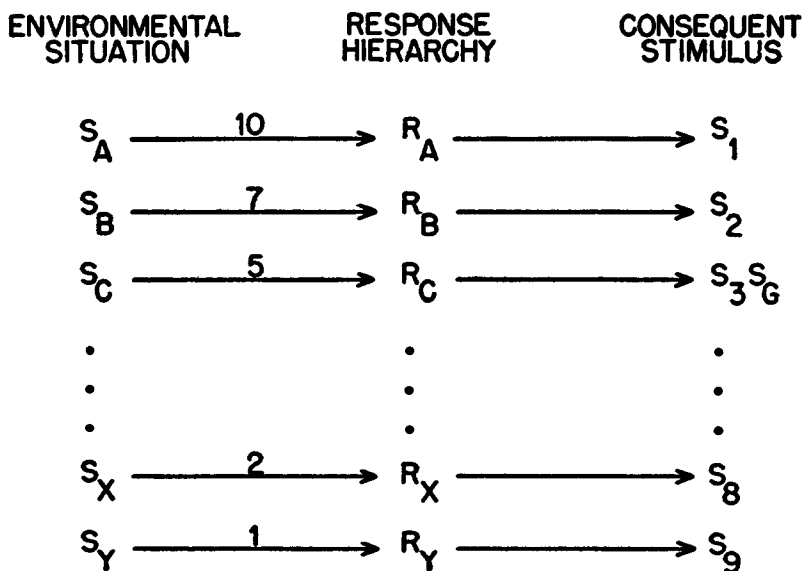
analyzing this behavior we may describe the organism as bringing to the new situation a variety of response tendencies differing in their relative strengths or likelihood of occurrence. This initial hierarchy of response strengths depends upon both innate and acquired or learned factors. The less experienced the organism the more will its initial repertoire of responses tend to consist of unlearned response tendencies. In more experienced organisms, on the other hand, the initial response hierarchy will depend heavily on the responses the organism has learned to make in similar situations in the past. Another way of putting this is to say that when first placed in the situation the subject will make a series of responses, the likelihood of occurrence of which will be a function of its past experience.

Figure 2 pictures the stimulus-response hierarchy of our selective learning situation. S_A, S_B, and S_C represent the stimuli from the three different alleys and R_A, R_B, and R_C the responses of orienting toward each of them respectively and locomoting (i.e., running) forward. S_X and S_Y represent other discriminanda that may elicit responses R_X and R_Y. The index numbers above the arrows joining the S's and R's indicate the different initial strengths of the responses and thus portray the hierarchal nature of the response repertoire. In selective learning the response chosen by the experimenter as the one to be learned is typically relatively low in the hierarchy. As a consequence the subject tends at first to make the stronger, erroneous responses. Learning involves the elimination of these erroneous responses and the prompt occurrence on each trial of the correct response.

While the notion or concept of response will be discussed in some detail later in connection with the treatment of instrumental conditioning some preliminary comments relevant to the kinds of response involved in selective learning situations will be helpful at this point. It will be observed from Figure 2 that each of the alternative responses has been designated, as is customary, by a single symbol, R_A, R_B, R_C. It should be understood, however, that each such response typically consists of a series or chain of movements or skills involving sensory-motor integrations of varying complexity. Indeed, insofar as behaviorally mature subjects are employed they bring with

them already acquired systems of perceptual and instrumental skill sequences. Certainly in the present maze type of situation the subject has already learned the receptor-orienting acts, e.g., fixating and looking at visual stimulus patterns, and the instrumental sequence of skills, such as walking or running forward, that are required.

ENVIRONMENTAL   RESPONSE   CONSEQUENT
   SITUATION   HIERARCHY   STIMULUS

S_A ——— 10 ———▶ R_A ——————▶ S_1

S_B ——— 7 ———▶ R_B ——————▶ S_2

S_C ——— 5 ———▶ R_C ——————▶ $S_3 S_G$

•   •   •
•   •   •
•   •   •

S_X ——— 2 ———▶ R_X ——————▶ S_8

S_Y ——— 1 ———▶ R_Y ——————▶ S_9

SELECTIVE LEARNING

Fig. 2. Diagrammatic representation of the response hierarchy in a selective learning situation. The numbers above the arrows indicate the different initial strengths of the tendencies for the various aspects of the environmental situation (S_A, S_B, etc.) to evoke the responses (R_A, R_B, etc.). In this situation R_C is the correct response since the presentation of the reward stimulus (S_G) follows its occurrence only.

In contrast to this maze type of situation the older problem-box types of selective learning involved novel devices or objects that required the subject to learn distinctive manipulative acts that often involved the perfecting of new, intricate sensory-motor skills. Thus these older devices involved a mix-

ture of both selective learning and the acquisition of skill. Such novel devices are deliberately omitted when our primary interest lies in the process whereby the alternative members in the response hierarchy change in their relative strengths and not in the study of the acquisition of skill.

Stimulus Events Consequent to Responding. This brings us to the consideration of the fourth and final component of selective learning, the stimulus events consequent to each response. In our learning situation with the three alleys, each of the three alternative responses, R_A, R_B, and R_C, takes the subject out of the choice chamber and brings it to a new situation or new set of stimulus events. While all three responses lead to end boxes which are usually more or less identical in their physical characteristics, only one of them contains what we have referred to as a reinforcer or goal event; the others do not. Thus one response, R_C, designated as the *correct* or *successful* response, leads to a reinforcing event while all other responses, including such responses as R_X and R_Y, do not lead to such an event.

We have used the terms reinforcement, reinforcer, and reinforcing event on a number of occasions, and it is now time that we clarified their use. Discussions employing such concepts are often likely to be quite confusing because of two very different kinds of meanings, one empirical and one theoretical, that they possess. As we shall employ the terms in the present discussion they will refer only to certain kinds of empirical events that may be designated on the basis of observation and will not have any theoretical reference. That is, in such simple selective learning situations the psychologist has observed that when certain types of environmental events accompany or follow a particular response, the response is more likely to occur on subsequent occasions. Omission of such events following a response, moreover, may be observed to lead subsequently to a lessened likelihood of its occurrence. Environmental events exhibiting this property of increasing the probability of occurrence of responses they accompany constitute a class of events known as *reinforcers* or *reinforcing events*. All environmental events not exhibiting this property fall into a different class that may be designated as *nonreinforcers*. By means of these

two classes of events it becomes possible to formulate the following law: Responses accompanied or followed by certain kinds of events (namely, reinforcers) are more likely to occur on subsequent occasions, whereas responses followed by certain other kinds of events (namely, nonreinforcers) do not subsequently show a greater likelihood of occurrence. This statement, which we shall refer to as the *empirical law of effect*, summarizes an observed relationship between the subsequent strength or likelihood of occurrence of a response and two different classes of effects or outcomes. It does not imply, it should be noted, any theory as to how the effects operate to bring about the change in response probability. As far as I can see, all psychologists, whether they support or oppose some form of reinforcement theory of learning, would agree to this empirical law of effect.

Vigorous disagreement among psychologists has arisen, however, over the theoretical interpretations of this law, particularly with regard to the nature of the action of a reinforcer. Thus a reinforcement theorist, such as Hull (1943) or Thorndike (1935), takes the law as a point of departure for his reinforcement theory that reinforcers strengthen the stimulus-response tendencies they accompany whereas responses not accompanied by reinforcers are not so strengthened and may even be weakened. Contiguity theorists, on the other hand, would deny that the increment in strength of any response tendency is due to, i.e., caused by, the reinforcing event. The function of the reinforcer according to a contiguity theorist such as Guthrie (1935) is that of protecting the increment in associative strength that is established merely as a consequence of the occurrence of the response to the stimulus. Other contiguity theorists such as Tolman (1934) offer still different interpretations as to the action of these reinforcers.

We shall not concern ourselves at this point with these hypotheses but instead will merely call attention to the fact that it is possible on the basis of experimental studies to specify four subclasses of such reinforcing events: (1) *Primary appetitional reinforcers*. Included in this group are environmental objects (e.g., food, water) which, by virtue of maintenance schedules controlled by the experimenter, evoke consummatory

responses in the subject. (2) *Secondary appetitional rein-forcers,* stimulus cues which in the past experience of the or-ganism have regularly accompanied the consumption of such needed objects as outlined above. Examples are the sight and odor of food as distinguished from food in the mouth. The sight of a water dispenser is another instance. (3) *Primary aversive reinforcers,* environmentally manipulated changes involving the cessation or reduction in intensity of an existing noxious stimu-lation (e.g., electric shock). And (4) *secondary aversive re-inforcers,* the cessation or reduction in intensity of acting stimulus cues that in the past experience of the subject have regularly accompanied a noxious stimulus. Each of these types of events may be identified as a *reinforcing event,* a *reinforcer,* or a *reward.*

Course of Selective Learning. To complete our analysis of simple selective learning, the course of such learning is marked by a gradual decrease in the frequency of occurrence of the in-correct, nonreinforced responses such as R_A, R_B, and R_Y, and a corresponding increase in the tendency to make the correct, reinforced response (R_C) promptly. Depending on whether the procedure known as the *correction method* or that called the *noncorrection method* is used, different indexes of the progress of learning are employed. In the correction method, in which the subject is permitted to continue behaving in the situation until the correct, goal-attaining response is made, two meas-ures are usually employed: (1) the time taken on each trial to make the correct response and obtain the reinforcement; and (2) the number of incorrect responses or errors made on each trial. Typically only the occurrences of responses R_A and R_B are counted as errors, not such responses as R_X or R_Y. The occurrences of these latter responses, however, take time and hence they do influence the time measure. In the noncorrection method, in which the subject is permitted to make only one of the three alternative responses, R_A, R_B, or R_C, on a given trial, the progress of learning is represented in terms of the per-centage of correct (or erroneous) responses that occur on suc-cessive blocks of trials. Learning is considered to be complete when the subject makes the correct response promptly, i.e., without making any erroneous responses. Usually some arbi-

trary criterion of learning is employed such as three successive trials without an error.

While the selective learning type of experiment has been extremely valuable in that it led to the formulation of the empirical law of effect and to such kinds of information as to what specific events constitute reinforcers, the relative effectiveness of different reinforcers, and so on, nevertheless its complexity has not been conducive to the development of any very systematic set of laws relating performance to the typical variables in learning behavior. For example, the results of investigations of the effects of different levels of motivation on performance in selective learning situations have been highly conflicting. Whereas some studies have shown that a higher level of performance is obtained with a strong drive than with a weak drive, others have exhibited the reverse of this, while still other investigations have revealed no difference. Similarly the effects of varying such variables as the intertrial interval, the magnitude and delay of the reward, have also been highly conflicting as far as the findings from selective learning situations are concerned.

COMPLEXITY OF SELECTIVE LEARNING AND NEED FOR SIMPLER LEARNING SITUATIONS

The complexity of this type of learning situation readily becomes apparent when we stop to consider that the learning curves represented by the response measures employed, time and errors per trial, reflect not only the increasing strength on successive trials of the correct response but also the simultaneous weakening of the competing, incorrect responses. Furthermore, in the correction procedure, the fact that the correct response occurs at the end of every trial indicates that at the moment of its occurrence it is the strongest response. Yet in the early stages of learning one or more of the incorrect responses typically occurs at the beginning of the next trial. While chance factors can account in part for this, the prevalence of these incorrect responses suggests that they recover spontaneously from the weakening effects of the failure of reinforcement in the interval between trials.

The response measures employed in this selective learning experiment may thus be seen to be a rather complex function of the operation of at least three different concurrent processes: (1) the increasing strength of the correct S-R tendency; (2) the successive weakening of the incorrect S-R tendencies; and (3) the spontaneous recovery of the latter between occurrences. What is needed, obviously, is an experimental situation that will permit the study of these processes in isolation from one another.

While various types of selective learning situations have continued to be employed, particularly the simple T maze, beginning about 1930 more and more use has been made of two simpler learning situations. One of these is the classical conditioning experiment originally devised by the Russian physiologists Bekhterev (1909, 1913) and Pavlov (1906, 1927). The second is the so-called instrumental conditioning experiment or, as Skinner (1935), one of its originators, termed it, operant conditioning. A number of different factors were responsible for the upsurge of interest in these simpler conditioning experiments, prominent among them being the series of theoretical and experimental articles (1929, 1930a, 1930b, 1931, 1932, 1934a, 1934b, 1937) published by the late Clark L. Hull after going to Yale in 1928. In these articles Hull took a radically different view of the conditioned reflex from that which had been put forward by Watson (1916). Watson had conceived of the conditioned reflexes as the elementary units from which more complex behavior was compounded by the principles of conditioning. In contrast Hull looked upon the conditioning situation as an experimental technique which, because of its simple and well-controlled nature, permitted the discovery of laws concerning basic processes of habit formation and habit breaking that enter into all learning. Moreover he believed that such laws would serve as a set of theoretical postulates from which deductions concerning more complex learning phenomena could be made. Hull's writings led to a large number of conditioning studies in this country, the findings of which provided the empirical basis for the theories put forward in his later books (1943, 1951, 1952).

In contrast to the more complex selective learning situations,

these classical and instrumental conditioning methods attempt to provide situations that permit the study of the acquisition of an S-R relation more or less in isolation from the weakening of other responses. Likewise they make it relatively possible to study and ascertain functions describing the weakening or extinction of a nonreinforced response uncomplicated by the simultaneous reinforcement of some other response. These techniques have also provided the means of investigating the process of spontaneous recovery of extinguished responses and the important phenomena of generalization of acquired and extinguished response tendencies. As will become apparent, however, these experimental methods, particularly instrumental conditioning, do not always provide for the complete isolation of these different phenomena.

The relation of these two simple types of learning to selective learning is of considerable interest, and it will be well worth our while to study them in some detail. Depending on the motivation-reward conditions two types of both classical and instrumental conditioning experiments may be distinguished: classical defense and classical reward conditioning and, corresponding to them, instrumental escape and instrumental reward conditioning. We shall begin with an analysis of the two types of instrumental conditioning, discussing first their relations to selective learning.

INSTRUMENTAL CONDITIONING

Selective learning, as we have seen, involves a hierarchy of alternative responses elicited by the various aspects or discriminanda of the environment. One of the responses is reinforced and all others are not reinforced. In both instrumental and classical conditioning the aim of the experimenter is to arrange for the occurrence of but a single response or response chain which is followed by reinforcement when the acquisition of an S-R relation is being studied and by nonreinforcement when its extinction is being investigated. In instrumental conditioning the to-be-learned response, as in the case of selective learning, is made to some aspect or part of the environment. However, instead of choosing a response relatively low in the

initial response hierarchy as is typically done in selective learning, the experimenter attempts to choose the strongest response, i.e., the one most likely to occur, as the one to be reinforced. In this manner he hopes to limit the investigation to the study of the strengthening of this single response. A variety of devices have been employed to maximize from the first the occurrence of this to-be-learned response and to minimize the occurrence of competing responses. One general technique that has been employed for this purpose is to have the object, manipulandum, or part of the environment to which the subject is required to respond be as obtrusive as possible or, as the Gestalt psychologists would say, stand out as a figure on a ground. Thus in the Skinner-box type of instrumental conditioning situation the lever that the subject must learn to depress in order to receive food is the only readily available manipulandum in the situation. Moreover, on the first trial or two the experimenter often smears it with wet mash.

In the maze type of instrumental situation the likelihood of competing responses occurring is reduced by having but a single straight-ahead alley instead of a number of alternative ones. Upon the raising of the door in the starting box the subject has merely to learn to orient ahead and to run forward in the single alley to the end box which provides the reinforcement. The Skinner-box situation and the simple runway situation that we have just described have been the most frequently used instrumental conditioning situations. When some form of appetitional need such as hunger is employed and an appropriate appetitional reinforcement (e.g., food) is used the situation is known as instrumental reward conditioning. In the escape type of instrumental conditioning some form of aversive motivation (e.g., shock) is employed, and escape from the noxious stimulation constitutes the reinforcement.

The behavior measures employed in these instrumental conditioning situations are concerned only with the properties or characteristics of the to-be-learned response; among them are the time it takes the behavior chain to get started, the time it takes some fraction or the whole chain to be run off, or as in Skinner's free-responding situation, the rate with which the response occurs in some period of time. While in theory no other

responses or response chains occur, in actual practice this ideal of only a single response is seldom if ever attained, and most instrumental conditioning situations are but limiting cases of selective learning in which the occurrence of competing responses is at best minimized.

The Concept of Stimulus in Conditioning. In addition to minimizing the occurrence of competing responses in instrumental learning the psychologist is also interested in controlling in as precise and invariant a manner as possible the stimuli that impinge on the organism's receptors from trial to trial. At this point a more careful consideration of the meaning or rather different meanings of the term stimulus is required for, if one may judge from a number of recent articles (Bakan, 1953; Luchins and Luchins, 1954) that have appeared in the psychological literature, confusion over this term is rife. Apparently the basis of the misunderstanding is that we tend to use the single word stimulus for a number of different things, depending too heavily upon the context and a highly cooperative reader to produce the appropriate differentiations. There is no point in going into the nature of the confusions themselves. Instead I shall try to clarify as best I can what needs to be understood. In the first chapter reference was made to the class of independent stimulus variables that the psychologist was said to manipulate. As used there the term stimulus meant the physical or world situation with its different aspects or features that the psychologist arranges to have present. The lever in the Skinner box, the alleys of different brightnesses and locations, the different types of flooring in the maze, the auditory stimuli introduced by means of oscillators, the triangular and circular forms of a discrimination apparatus are all examples of such environmental or, as I shall designate them, situational stimuli. Depending upon the purpose of the investigation all of these things can and may be manipulated by the experimenter. Thus the brightness of the alleys may be different or identical, the forms can be large or small, the auditory stimuli can be varied in their frequency and amplitude, and so on.

But the presence of a particular situational stimulus does not guarantee that it will also be a stimulus for the organism at the

moment of responding. Here we have used the term stimulus in a second, different sense. In his *Principles of Behavior* Hull differentiated between these two concepts of stimulus by the use of the terms potential stimulus and actual stimulus. The different manipulable bodies or constellations of energy in the physical environment provide potential stimuli for the organism, but only those actually *impinging on* and *exciting the receptors* at the moment of responding are actual stimuli for the response. These latter stimuli, which we shall designate as effective stimuli, depend not only upon the manipulations of the experimenter but also on the subject's receptor capacities and receptor-orienting acts. Thus situational stimuli to which the receptor is not sensitive, e.g., subthreshold visual intensities or very high-frequency air waves, cannot be effective stimuli for the subject. Likewise, if a receptor is not oriented so as to receive a situational stimulus, the latter will not *at that moment* be an effective stimulus for the subject. Furthermore, it should be realized that the same situational stimulus may on different occasions produce slightly different effective stimuli. Thus in a form discrimination experiment with animals the effective stimulus the subject receives from a triangular or circular form may vary considerably from one exposure to another depending upon the receptor adjustment of the subject. Indeed the learning of receptor-orienting responses that will provide discriminably different effective stimuli to the subject is an important part of such learning.

In the simple type of learning situations we are considering, however, the experimenter is not interested in the acquisition of such receptor-orienting acts, which is itself a fairly complicated process of selective learning. Instead he strives to arrange his experimental setup in such a fashion that no new, special receptor-orienting act is required to be learned and so that the sample of situational stimuli or stimulus elements impinging on the subject's receptor surfaces will show a minimum of variation from trial to trial. At best, however, this variation is considerable, especially in the runway and Skinner-box types of instrumental learning situations in which the subject is free to move around.

Returning to our discussion of the concept of stimulus, one

further usage of the term in psychology should be mentioned here. In this instance the referent is a nonobservable event, usually intraorganic, the presence of which is inferred on the basis of known physiological laws. One example is the excitation of proprioceptors by muscular action, or the concept of response-produced stimuli. On the basis of physiological laws the psychologist assumes that such distinctively different responses as turning the head to the left or to the right produce distinctively different proprioceptive stimuli to the immediately following responses. Other examples of such intraorganic stimuli are those that accompany or result from appetitional-need states such as hunger, thirst, and sex. Unfortunately the physiological laws relating these latter internal events to either antecedent environmental events or to behavior are not so well established with the consequence that psychologists have had to be much more theoretical in their use of such drive or need stimuli.

In summary, consideration of the concept of stimulus reveals that it has at least three different classes of referents: (1) *Situational stimuli*—the realm of physical objects or events in the environment that may be specified quite independently of the responding organism and which are under the direct control of the experimenter. (2) *Effective stimuli*—the particular sample of the totality of events of the first class that are acting upon and exciting the receptors at the time of a response. These are to a considerable extent dependent upon the organism's receptor-orienting behavior. (3) *Intraorganic stimuli*, the properties of which are specified on the basis of known physiological laws or on the basis of hypothesized internal relations. These latter stimuli are also dependent upon the organism.

The fact that the behavior of the subject at any moment depends entirely on the second and third class of stimuli, which are not always directly under the control of the experimenter, does not mean that it is not possible to discover laws relating behavior to these various types of stimulus variables. By means of the physiological laws concerning interoceptors, laws concerning the receptivity of the exteroceptors, and experimental techniques calculated to insure the reception of a particular

set of situational stimuli, it is possible to specify and keep constant, within limits, the sample of stimulus events from one trial to another. The classical and instrumental conditioning techniques attempt to provide just such controlled conditions. Either they arrange to have the particular aspect of the environment to which the response is to be made stand out in as striking a fashion as possible, or they attempt to control the receptor-exposure adjustment of the subject at the time of the response. As we shall see the classical conditioning techniques have been somewhat more successful in this regard than the instrumental conditioning methods.

The Concept of Response in Conditioning. The concept of response also requires some consideration, although the usual treatment of it suffers more from incompleteness than from confusion as in the case of the stimulus. A number of writers (Guthrie, 1930; Tolman, 1932; Murray, 1938; Hull, 1943) have carefully distinguished between two different conceptions of response, one called molecular and the other molar. According to this distinction behavior items may be described either in molecular terms as movements or actions, i.e., patterns of muscular contraction, or in molar terms as acts. The latter are specified in terms of what changes they produce in the immediate environment or in the relation of the organism to the immediate environment.[1] Differences in the detailed movements or patterns of muscular activity from one occurrence to another are ignored, and all instances that produce the same environmental change are treated as a single response class. Examples of this molar class of responses are such acts as pressing down the lever in the Skinner box, orienting toward and running down a maze alley, jumping toward a stimulus card, and so on.

For the most part learning psychologists employ this second class of response concepts. Certainly all the responses involved

1. Attention should perhaps be directed to the point that, as here defined, acts do not have any reference to the subsequent attainment or nonattainment of goals but strictly only to the immediate effect on the environment that is present at the time of the response. In this respect this conception is quite different from that of Tolman who seems to include reference to later achievements, including goal attainments, in his definition of an act.

in instrumental conditioning and selective learning situations would be classified as acts rather than movements, and contrary to common belief, even the responses employed in classical conditioning appear to fall into this category. Thus in eyelid conditioning the different patterns of muscular contraction of the eyelid muscles are ignored, and the response class is defined in terms of the varied movements that get the lid closed a certain critical amount. Likewise leg flection is defined in terms of lifting the leg from the grid or floor with no account being taken of differences in muscular contraction from one occasion to the next. That is, as long as the measuring operation defining the response variable ignores variations in the pattern of muscular action involved and specifies the response only in terms of some environmental outcome or change in the organism-environmental relation, it falls into the class of responses called acts as distinguished from movements.[2]

In our discussion of the alternative responses in selective learning mention was made of the fact that each response involved a chain or sequence of acts. We now need to take a closer look at this aspect of behavior. The upper sequence in Figure 3 presents a modification of an analysis made by Keller and Schoenfeld (1950) of the behavior involved in the freeresponding Skinner box. The first four stimulus components represent the stimulation received from the lever at different stages of the sequence; the successive responses in the sequence are orienting and locomoting toward the lever, rising as it is reached, and pressing down. Upon hearing the noise of the food delivery apparatus, to which it has already been conditioned, the subject lowers itself to the food cup and seizes the food pellet. At the completion of the eating of the pellet the chain is repeated and the measure of response strength is the rate of responding, i.e., the number of such cycles or chains of responses per given period of time. In the case of the discrete trial technique in which the lever is removed upon the delivery

2. Instrumental responses differ in this respect from classical conditioned responses in that the latter always involve the activation of the same system of muscles from one occasion or trial to the next whereas instrumental acts may, in part at least, involve different muscle systems. Thus the lever may be depressed by the left paw on one trial and by the right on another.

of the food pellet and then reinserted at the beginning of the ensuing trial, the response is measured in terms of the time required for the portion of the chain preceding the segment
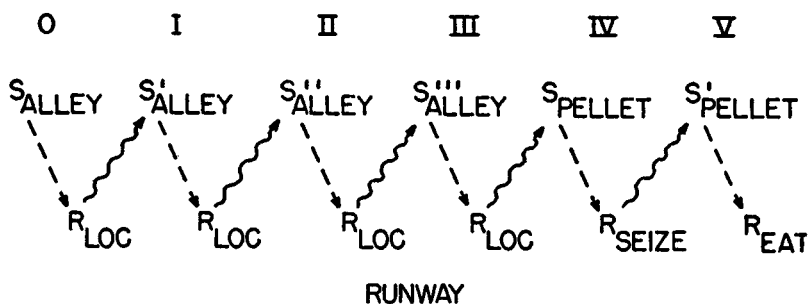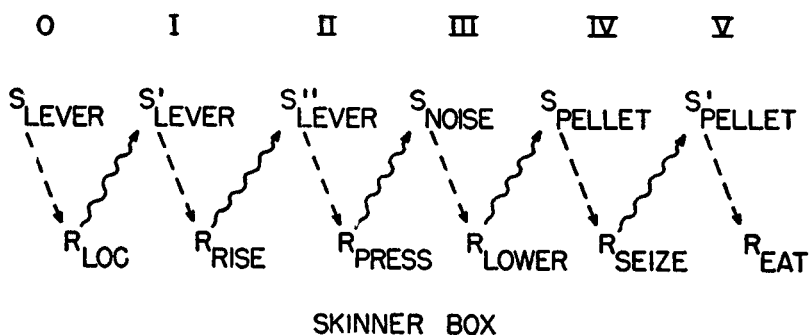
Fig. 3. Diagrammatic representations of a *heterogeneous chain* of responses (Skinner box) and a *homogeneous chain* (runway). Note that the links of the heterogeneous chain differ distinctly from each other, while those of the homogeneous chain, for a considerable segment, are more or less similar. The primes indicate slight changes in the stimulus complex due mainly to changes in orientation since the corresponding physical stimuli are essentially uniform. The arrows with wavy shafts represent physical causal relationships; the arrows with broken shafts represent S-R tendencies in the process of formation. The Roman numerals denote arbitrary time segments of what is essentially a continuous behavioral flux.

marked III to run off. As Keller and Schoenfeld have pointed out such an analysis of what is essentially a continuous flux of behavior is somewhat arbitrary, and probably more component reflexes could have been specified.

The type of chain represented by this situation, in which the successive stimuli and particularly the responses are distinctly different from one another, will be referred to as a *heterogeneous chain*. In contrast to this is the *homogeneous chain* in which both the stimulus complex and the responses for a considerable segment of the chain are more or less similar. Our runway situation provides a good example of such a homogeneous chain. As the lower sequence in Figure 3 shows, the initial response is orienting and locomoting down the pathway and the final segments involve seizing the pellet and eating. As the subject proceeds, the visual stimulus complex changes slightly depending in part on the source of illumination, although the experimenter attempts to make the visual environment along the runway as uniform as possible.

It is readily apparent from the preceding analysis that even the homogeneous type of instrumental response chain is not simple and that measures of response strength obtained in such a situation involve a number of different, component S-R links. Even if one confines the sector of the response chain that is measured to fairly homogeneous S-R links, such as would be involved in segments O to III in the runway chain, one has a measure that represents some kind of average of the strengths of a number of similar S-R components. When one considers the additional fact that it is very difficult, especially in the early stages of learning, to eliminate competing responses completely from the instrumental conditioning situation, it becomes obvious that learning curves obtained in this type of situation reflect much more than the changing strength of a single, fairly simple S-R tendency.

Still another drawback of the instrumental learning situation is that at the start of the experiment the correct, reinforced S-R tendency already possesses some finite strength, usually considerable if it is the strongest in the hierarchy. As a consequence a portion of the course of its development or acquisition has already occurred and thus cannot be studied. Considerable experimental ingenuity is required to provide a minimum of transfer from past learning so that the to-be-learned response is low in strength and yet sufficiently stronger than any other responses as to have the occurrences of the latter

minimal. Far too little attention has been given to this requirement by experimenters in the past.

CLASSICAL CONDITIONING

Turning now to classical conditioning we find that both the stimulus conditions and the response are much simpler and better controlled than in instrumental conditioning. In classical conditioning the occurrence of the response that is to become connected or associated with a new stimulus is under the strict control of the experimenter. This response is an unconditioned or innate reflex elicited by the introduction into the environment of the so-called unconditioned stimulus. In the defense type of classical conditioning this reflex is evoked by some form of noxious stimulus. Examples are the elicitation of limb flection by shock applied to the paw or finger and the closure of the eyelid to a puff of air striking the cornea. The upper graph in Figure 4 represents this classical defense type of conditioning situation. The steplike rises of the horizontal lines represent the points of onset of the conditioned and unconditioned stimuli, and the falls their cessation. The unconditioned reflex is represented by the solid arrow leading from the unconditioned stimulus, an air puff to the eye, and the new association being acquired is represented by the broken arrow leading from the conditioned stimulus, a tone or light. Learning or conditioning is revealed by the increasing capacity with successive training trials of the previously neutral stimulus to elicit the response originally evoked only by the unconditioned stimulus.

In classical defense conditioning the experimenter limits the response to a single brief act, e.g., flexing the limb or closing the eyelid, by arranging for the noxious, unconditioned stimulus to be present for only a brief interval. As a consequence the single response that has time to occur is followed immediately by the reinforcing event, cessation of the noxious stimulus. Thus trial-and-error activity is short-circuited, and in contrast to the more or less extended chain of acts involved in instrumental conditioning the response is very brief and relatively simple.[3]

3. A variation in the procedure employed in classical defense conditioning consists in omitting the unconditioned stimulus if the conditioned response an-
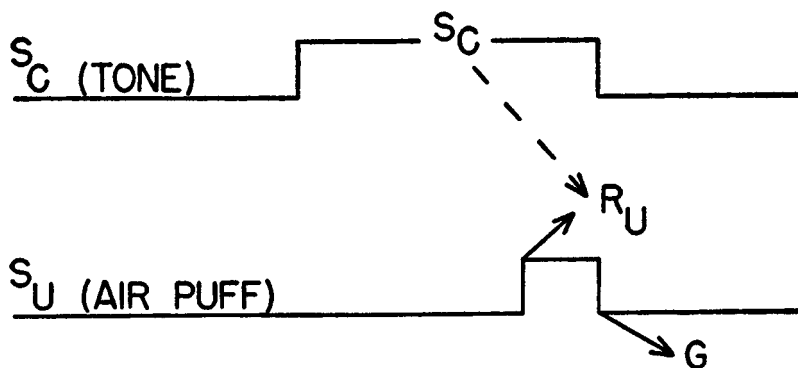
CLASSICAL DEFENSE CONDITIONING
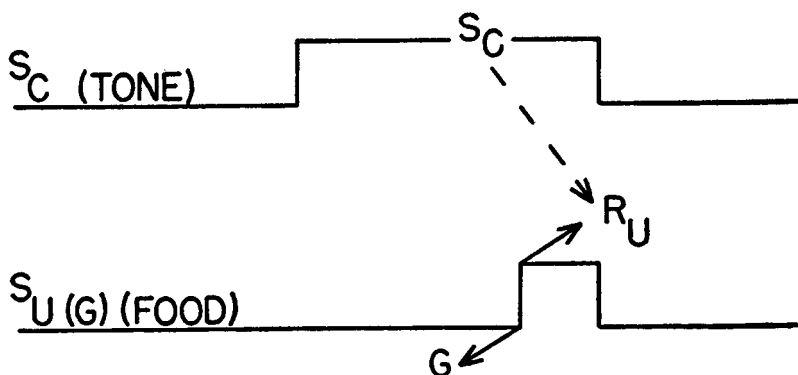
CLASSICAL REWARD CONDITIONING

Fig. 4. Diagrammatic representations of two types of classical conditioning. The steplike rises and falls of the horizontal lines represent the temporal sequence of stimulus onsets and offsets. In both types of conditioning the S_U is presented shortly after the onset of the S_C to insure the close contiguity of the S_C and the R_U. The two situations may be seen to differ in regard to the nature of the rewarding event (G): in classical defense conditioning G is associated with the offset of the S_U, while in classical reward conditioning G is associated with the onset of the S_U.

Classical reward conditioning, of which Pavlov's salivary conditioning is the best-known example, is represented in the lower part of Figure 4. As in the case of classical defense conditioning the experimenter controls the occurrence of both the response and the stimulus to which it is to be conditioned. The response in this instance is elicited by the reinforcing object, food, which serves a dual function, that of eliciting the unconditioned reflex and providing the reinforcing event. Instrumental responses to objects or manipulanda in the environment are minimized by not having any readily available and by a period of prior adaptation to the experimental situation in which the animal is trained to stand quietly in the stock. It may be noted that the subject is usually required to make an instrumental response to the reinforcing (food) stimulus (i.e., pick it up) but that no instrumental response is necessary to bring this stimulus into the situation. In some of his earliest experiments Pavlov even eliminated this instrumental response by blowing the food directly into the mouth of the dog. Whether or not this instrumental response was required, Pavlov limited his observations and measurements to only a fraction of the total response pattern made to the conditioned stimulus, namely, the amount of salivary secretion.

Controlling as he does the moment of occurrence of the unconditioned response, the experimenter is also able in classical

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ticipates its scheduled time of occurrence. This procedure has been designated avoidance conditioning because the conditioned response avoids the unconditioned stimulus. Starting out as standard classical defense conditioning in which the unconditioned stimulus occurs on each trial, avoidance conditioning gradually shifts to a partial reinforcement situation, i.e., one in which the occurrence of the primary reinforcement is intermittent.

A type of avoidance conditioning that involves a learned instrumental act has also been employed by a number of investigators (e.g., Hunter, 1935; Brogden, Lipman, and Culler, 1938; Sheffield, 1948; Kessen, 1953). In this type of experiment subjects are first trained in a selective escape learning situation to make some response in order to escape the noxious stimulus, e.g., rotate a wheel to escape shock. Following this training a tone is presented at a fixed interval prior to the onset of the shock, and the subjects learn to respond to the tone as a signal by approaching the wheel and rotating it before the shock occurs. Again an anticipatory response precludes the administration of the noxious unconditioned stimulus with the consequence that the schedule of primary reinforcements becomes intermittent.

conditioning to provide for the occurrence of the conditioning stimulus in a controlled temporal relation to it. This not only permits keeping the temporal phase of the conditioned stimulus at the moment of the response constant from trial to trial but also allows for its systematic variation. The instrumental conditioning situation does not allow for this since both the reception of the conditioned stimuli and the relation of their temporal phases to the responses with which they are associated are controlled not by the experimenter but by the subject's own actions. In the classical situation the experimenter also attempts to present the conditioned stimulus under constant conditions of reception. Thus the human subject is instructed to fixate a definite point in the visual field just before the conditioned stimulus is presented, or in the case of animal subjects the experimenter may employ a conditioned stimulus that does not vary to any great extent with changes in the subject's receptor orientation, e.g., a shift in the general illumination of the environment or the onset of a nonlocalizable tone.

Finally, attention should be called to the fact that classical conditioning is really an inherent part of instrumental conditioning. This is shown most clearly by the upper diagram in Figure 5, which, it will be noted, is an elaboration of the terminal portion of an instrumental reward chain. The solid arrows, marked by roman numeral I in this diagram, represent the instrumental learning and the broken arrows, marked by roman numeral II, the classical conditioned response. In instrumental conditioning the experimenter has typically recorded and measured only one of the two kinds of response, the instrumental, perceptual-motor skill sequence. But in addition to learning the instrumental response the subject also presumably becomes conditioned to make anticipatory feeding responses to preceding stimulus events that have the appropriate temporal relation. This latter is the classical conditioned response and in the case of food reward consists in that fraction or portion of the complex pattern of eating activity that can occur without the presence in the mouth of the food. Similarly, in instrumental conditioning experiments involving some form of external noxious stimulation as the motivating condition the stimulus cues in the alley or box become conditioned to evoke

autonomically controlled emotional or fear components of the total response originally evoked by the noxious stimulus. As the experimental studies of Miller (1951) have shown, such classical fear conditioning in an instrumental conditioning

A.
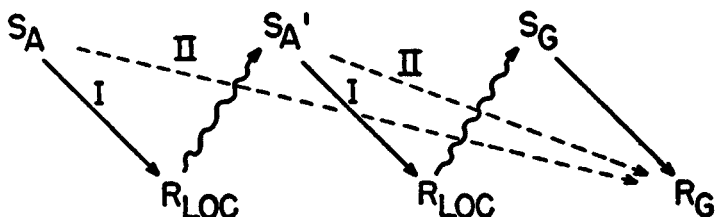
B.
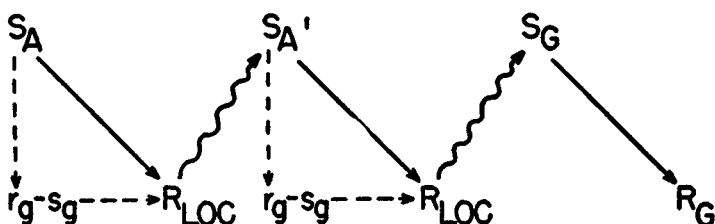
Fig. 5. Diagrammatic representation of the conditioning processes involved in instrumental learning. A. *Early in training.* This diagram shows how classical conditioning of the consummatory response (R_G), represented by arrows marked II, is an integral part of instrumental learning. The arrows marked I represent the instrumental conditioning of the locomoting response. B. *Later in training.* The stimulus cues of the alley (S_A, S_A') are assumed to evoke an anticipatory, fractional portion of the consummatory response, denoted r_G. The interoceptive cue (s_G) produced by r_G, in turn, is assumed to acquire a habit loading for the instrumental locomoting response.

situation can provide secondary motivating and reinforcing conditions for learning.

Thus it is apparent that the subject in instrumental conditioning experiments not only learns to make a particular in-

strumental response but also acquires an expectation or anticipatory response that prepares it for dealing with the impending event. Furthermore, it may be presumed that such classical conditioned responses, through the interoceptive stimulation they provide, play an important role in determining the strength of the instrumental response. The lower diagram in Figure 5 shows one way in which such classical conditioned responses have been conceived to operate to determine the instrumental response. In terms of the runway situation the stimulus cues at the beginning of the alley are assumed to become classically conditioned to evoke a fractional component (r_g) of the consummatory response (R_g). The interoceptive stimulus cues (s_g) produced by this classical conditioned response in turn are assumed to become conditioned to the instrumental running response and thus become a determiner of it. A more detailed treatment of still another way in which such anticipatory response mechanisms may be conceived to determine the strength of the instrumental response will be considered later (Chapter 5) in connection with the topic of incentive motivation.

This analysis of the relation of classical to instrumental conditioning suggests, it should be noted, that classical conditioning is considerably less complex than the instrumental type. On this basis it would appear that the classical situation is likely to be a more satisfactory source of information concerning the basic laws governing the strengthening and weakening of S-R tendencies.

SUMMARY DESCRIPTION OF SIMPLE LEARNING SITUATIONS

In summarizing this analysis of the different experimental techniques that have been employed to investigate simple learning phenomena, the following characteristic features should be noted. Learning situations involve the repeated presentation of the same or an essentially similar environmental situation to which the organism responds with one or more distinguishable kinds of behavior. In their turn these responses bring or are followed by new and different types of stimulation either by

virtue of the fact that they produce a change in the environment or because they take the organism to a new environment. Learning is revealed by the changes that occur in the behavior of the subject on the successive trial occasions. The psychologist records and measures these changes in the properties of behavior and attempts to ascertain the factors or variables that are related to them. While primarily interested in the laws relating the changing response measures to the successive trial occurrences (the so-called learning functions), the learning psychologist is interested in all of the variables that determine behavior in these situations. These determining variables and the laws involving them may be grouped into the following classes depending in part on their locus:

1. Variables in the stimulus situation to which the to-be-learned response is made, such as the intensity and duration of the conditioned stimulus in classical conditioning and the number and complexity of the manipulanda in instrumental and selective learning.

2. Variables in the stimulus situation following the occurrence of the to-be-learned response. Examples are the magnitude of the reinforcer or the time of its delay.

3. Variables related to the responses occurring in the situation such as the amount of work involved in the response and the time interval between successive response occurrences.

4. Variables related to certain states of the organism that are manipulated by variation of antecedent conditions in the environment. These may be subdivided into the learning (practice) variable itself, i.e., the number of reinforced or non-reinforced trials, the motivational variables such as the time of deprivation of food or water, and neuroanatomical and physiological variables involving prior modification of the organism by operative procedures or by the administration of drugs.

In the remaining chapters we shall examine the experimental findings obtained by the classical and instrumental conditioning techniques with a view to formulating a theoretical structure that will adequately account for the variations of response in these two situations. We shall not have time to consider all of the relevant variables, but the most important ones, particularly

those about which we have the most adequate data, will be considered. Having formulated a theoretical model we shall then attempt to give a few examples of how it may be extended to more complex types of learning—such as selective learning in animals and paired-associate learning in human subjects. In the presentation major emphasis will be given to the difficulties and problems that the psychologist faces in attempting to accomplish this task.

3. Acquisition Curves of Conditioning

IN THE NEXT FEW CHAPTERS we shall be concerned with the empirical findings that have come out of the simple conditioning situations described previously. Our primary interest in these data lies in the implications they have for the construction of a theory that will serve to derive and integrate the different facts and laws they reveal. In addition to this theoretical objective, however, I am also interested in exhibiting and discussing some of the problems and issues that arise in connection with such attempts. This will entail considering at some points alternative assumptions and the experimental evidence pro and con. It will also afford an opportunity to suggest needed points of investigation.

For a number of years, in fact ever since I was a graduate student at Yale University in the early thirties, my theoretical interests have been closely aligned with those of the late Clark L. Hull. By virtue of a rather prolonged period of conditioning then, I am quite partial, some would undoubtedly say biased, toward the stimulus-response type of theoretical analysis that Hull pioneered. It will come as no surprise then that the approach to be taken in the present work is basically similar to that of Hull and that many of his theoretical assumptions and mechanisms will be employed. However there were a number of matters on which Hull and I never did agree. Some mention of these at the beginning will help to focus attention on the emphases or features of the present approach that are different.

First let me say that the use of the formal style of hypo-thetico-deductive presentation such as is represented by Hull's *Mathematico-Deductive Theory of Rote Learning* monograph (1940) never appealed to me for the reason that it tended to give a very misleading impression of the state of development

of theory in psychology. Moreover, as Bergmann and I pointed out in an article on operationism and theory (1941) Hull's theorizing was not really of the type that began with the introduction of an axiomatic system which was later coordinated to an empirical system of concepts, however much it might look as though it were. Rather, as we showed, his theory was of the intervening variable type in which abstract concepts were introduced in terms of the empirical variables as a means of organizing the laws holding among them. Following this article Hull accepted this conception that his theorizing was really of this intervening variable form, and he explicitly employed it in his *Principles of Behavior* (1943).

A second matter on which Hull and I differed, although the difference was one of degree, was the extent to which, after defining a hypothetical intervening variable in purely quantitative terms as a mathematical function of some experimental variable, further neurophysiological specifications should be made. Hull was particularly prone to add such further specifications suggesting the possible locus, structure, or functioning in the nervous system of these constructs. His justification always was that such conjectures provided experimental hints to physiologically oriented psychologists interested in making such coordinations of our knowledge.

Now my interest in the integration of knowledge from different fields would, I think, make me one of the last to deny the potential value of such attempts. However, I would insist that the added physiological concepts introduced by the psychologist should be significant ones, by which I mean that they should be concepts that appear in a network of physiological laws. Unless such is the case one does little more than add a physiological name. Moreover, there is no little danger that such physiological notions will mislead or at least distract from the significant aspects of the mathematical portion of the theory. Indeed there is probably no better instance of this than that provided by Hull's concept of habit strength (H). After defining habit strength as a mathematical function of the present stimulus and antecedent environmental conditions, Hull then identified it further with the notion of a receptor-effector connection in the nervous system (Hull, 1943). Just what

beneficial "surplus meanings" accrued to the mathematical concept of habit as a result of this added physiological connotation has never been made clear to me. That it did provide for additional meanings is amply shown by the extent to which critics used them to characterize the whole theory as based on an outmoded conception of the functioning of the nervous system or, worse still, to deride it as a mechanistic, telephone-switchboard theory of behavior.

It is perhaps unnecessary to add that it was only the mathematical definitions of Hull's intervening variables that provided for the derivation of the empirical laws of conditioning. As so defined these constructs have no relation whatever to such issues as whether learning involves some kind of organization among sensory processes or whether it is to be conceived in terms of changes in the resistance of neural connections mediating between afferent and efferent processes. It is only by virtue of this quite superfluous neurophysiological speculation that Hull's theorizing ever got enmeshed in such issues. For my part I would prefer to remain neutral on such matters. I do not in the least find it difficult to conceive of both types of neural organizations being established in learning. Let me hasten to add here that I am not advocating that behavior scientists eschew all physiological concepts. That all behavior has physiological correlates seems as certain to me as anything I know. Hence I should not hesitate to employ fruitful physiological concepts whenever I thought they might help. At the present stage of development they are not, in my opinion, very useful.

A third major point of difference between Hull's point of view and mine did not develop until the writing of the *Principles,* when Hull adopted a reinforcement theory with a very specific physiological hypothesis as to the nature of reinforcement. It is interesting to recall that in his theoretical articles written in the early thirties Hull was not a reinforcement theorist. He primarily used Pavlovian concepts and when he employed the term reinforcement he used it in the same methodological, nontheoretical sense that Pavlov did. Following a special review of Thorndike's book *Fundamentals of Learning* (1932) for the *Psychological Bulletin* in 1935, Hull shifted to a position that may be characterized as a general reinforcement

theory based on the empirical law of effect. This position is best represented in his 1936 presidential address to the American Psychological Association. There, after postulating that S-R associations followed by a reinforcing state of affairs increase in strength, Hull went on to specify what he meant by such states of affairs. As he stated it in his third postulate, which I quote, "A characteristic stimulus-reaction combination (Sg→Rg) always marks reinforcing states of affairs . . . The particular stimulus-response combination marking the reinforcing state of affairs in the case of specific drives is determined empirically, i.e., by observation and experiment" (1937, p. 16).

In a theoretical article on discrimination learning published the same year I took essentially the same position, as the following quotation shows: "This (reinforcement) principle . . . assumes that if a reaction is followed by a reward, which may be defined in terms of the occurrence of a final or consummatory response, the excitatory tendencies of the immediate stimulus components are reinforced or strengthened by a certain increment" (1936, p. 430). As was pointed out further this specification of reinforcers was a general one involving no assumption whatever as to either their physiological nature or their mode of action. In this latter connection it should be noted that no assumption was even being made as to whether the increment in associative strength was directly due to the reinforcer or whether the increment resulted merely as a consequence of the act occurring and the role of the reinforcer was that of insuring its continuance. The bare assumption was made that the end result of the action of a reinforcer was a net increase in the particular S-R tendency it followed. In this sense, it will be noted, even Guthrie would qualify as a general reinforcement theorist.

Whereas I have continued to hold to this general conception of reinforcement, Hull in the *Principles* shifted to a quite different conception. In this new formulation he became a true reinforcement theorist in the sense that he assumed that the increment in associative strength or, as he termed it, habit strength depended directly on the occurrence of the reinforcer. Furthermore, he specified the nature of reinforcers physiologically in

terms of a reduction in drive strength or a reduction in the strength of a drive stimulus. Here again Hull's proclivity for tacking a physiological conception on to his mathematical theory got him into a lot of unnecessary trouble. One critic (Hilgard, 1948) even went as far as to state that this portion of his theory was so central that if it was not valid the whole system would collapse. Actually it was not essential to the mathematical portion of the theory at all.

Still another difference between Hull's theorizing and my own attempts was the relatively greater willingness on Hull's part to go ahead and hazard a set of theoretical postulates on the basis of a minimum of empirical data. Hull was not in the least afraid of being wrong, and he held to the point of view that one should proceed with the best hypothesis available at the moment. Experimentation, he believed, would provide the necessary corrective for any mistakes. In contrast I have always been inclined to keep my theorizing much more closely tied to the available empirical data. Again, however, this difference is only one of degree, and it is possible from a few of the statements made by Hull, particularly some that occur in the prefaces of his books, to misinterpret the object of his work. A case in point is the recent vigorous criticism of Hull's theorizing that came out of the Dartmouth conference on learning theory (Estes et al., 1954). One of the major criticisms of Hull was to the effect that he regarded the principles and laws formulated in the *Principles of Behavior* as generalizable without qualification or addition to all human behavior of whatever degree of complexity. Working as closely as I did with Hull during the period of preparation of this book I believe I was in a good position to know how he structured the task he set himself. As I have always understood it and have described it in a number of articles (Spence, 1944; Spence, 1948; Spence, 1953a), what Hull attempted to do in the *Principles* was to develop a theoretical structure that would serve to derive and interrelate the empirical laws from two simple learning situations, classical and instrumental conditioning, nothing more. He did assume that these laws, along with others, would serve as a theoretical structure to account for more complex learning behavior. However, he knew that he stood at the very beginning of such an under-

taking, and he was well aware that the first step was to develop a theory that would work for existing conditioning data. Apparently the misunderstanding of the critic was based on a hypersensitivity to the somewhat expansive statements that appeared in the *Principles*. Paradoxically enough the running criticism of the Gestalt psychologists throughout this period was that Hull's theorizing was concerned with too narrow a range of phenomena, namely, just classical and instrumental conditioning.

But regardless of what Hull's conception of his theory may have been, I would like to make it very clear that my own conception of what we are trying to accomplish is to provide a theoretical structure that will permit the derivation of the different empirical functions found in the several types of conditioning experiments. That is, the initial objective is to develop a set or system of concepts and postulated interrelations which, in combination with the different initial and boundary conditions specific to each experimental situation, will provide for the deduction of the many specific empirical relations that have been found in conditioning studies. It is hoped further that this theory, along with the necessary additional composition rules, will permit one to account for more complex learning phenomena.

TYPES OF LEARNING CURVES: INDIVIDUAL VERSUS GROUP CURVES

Turning now to the consideration of the data of conditioning studies and their implications for a theory of simple behavior phenomena, we shall begin with the so-called learning or acquisition curves. This class of laws pictures the manner in which the behavior of a subject changes as a function of the number of reinforced conditioning trials. An examination of the existing data in the literature reveals that the vast majority of curves represent the pooling or averaging of individual data. Apparently even these simple conditioning techniques have provided so little control of the organism that the variability of individual measures has been such as to obscure any lawful relation present. Faced with this difficulty investigators have usually

resorted to pooling the measures of a large group of subjects, the mean or median response measure of the group on each trial being plotted against the ordinal number of the trial or blocks of trials. Unfortunately this type of data is frequently of little use to the theorist for, as may be shown, such averaging of individual data often produces considerable distortion with the consequence that the group curve does not accurately reflect the individual curves. Indeed some time ago Merrell (1931) demonstrated that the average curve for some functions, for instance the logistic and exponential functions, cannot be of the same form as the individual curves except under special conditions that are seldom if ever met in conditioning experiments. More recent treatments of this same point have been made by Sidman (1952) and Bakan (1954).

Fortunately, there are a number of alternative procedures for treating data from a group of subjects that provide more or less satisfactory functions, i.e., functions that are not so variable as those for individual measures but which still are representative of individual curves. The distortions produced by averaging the data of a group of subjects result from the fact that the individual subjects usually differ markedly not only in their initial and final levels of performance but also in their different rates of learning, i.e., their different rates of approach to the performance asymptote. One method of averaging that attempts to take account of differential rates of learning is the Vincent method of combining individual learning curves (Vincent, 1912). In this method each individual curve is divided into an equal number of parts, e.g., tenths, and after each individual measure for the successive parts is determined a group curve is obtained by averaging these values. A number of writers (Hilgard, 1938; Melton, 1936) have called attention to certain sources of error introduced into the data by this method, the most serious from the point of view of the form of the curve being the necessarily arbitrary criterion of complete learning.

Instead of a curve based on the average of all of the subjects of a group, curves of learning may be determined by using only "like" or homogeneous subjects. A number of different criteria of likeness have been employed. For example, Hilgard and Campbell (1936) made use of the shape of the individual

curves. After classifying the individual subjects into subgroups on the basis of the shapes of their conditioning curves, these investigators obtained average curves for these like subjects. Another criterion, which I have employed, has been to select subjects on the basis of the similarity of their response measures for the total learning period (Spence, 1952). Thus, in the case of the frequency measure in classical conditioning, after observation of the subjects' scores in terms of the total number of conditioned responses occurring in a given number of trials, e.g., 100, groups of like subjects were formed in terms of those that fell within a small range of scores, such as from 20 to 30 conditioned responses or from 50 to 60 and so on. Further refinements of this procedure are, of course, possible. Thus, instead of using such an over-all measure based on the total period of learning one may obtain measures for each subject at the beginning, at some intermediate point, and at the end of learning. Groups of subjects can then be formed that are alike at all three points. In this instance the composite curve is one in which the subjects are more or less alike not only in their performance levels at the beginning and end of learning but also in their rates of learning.

A final method of overcoming the defects of averaging the measures of different subjects is to employ, whenever possible, the curves of individual subjects. Smoothing of individual curves may be accomplished by averaging measures on blocks of trials, providing blocks of a relatively small number of trials are employed. The use of large blocks of trials is not satisfactory unless the learning is very prolonged because it is likely to cover up any brief changes in acceleration that may be present. For the most part, when interested in the form of any empirical learning curve we shall depend upon individual curves, Vincentized group curves, and composite curves based on like or homogeneous subjects as described above.

CURVES OF CLASSICAL CONDITIONING

Frequency Curves: $R_p = f(N)$. Let us now turn to the data on conditioning curves. Because of the relative simplicity of classical conditioning we shall begin with the findings from this

type of experiment, and because it has been by far the most commonly used measure of response strength we shall first present curves based on the measure known as frequency or percentage of response. By specifying certain amplitude and latency characteristics the experimenter is able to count the number of trials on which a response meeting these specifications occurs. Thus in eyelid conditioning it has been customary to identify conditioned responses as any closure of the eyelid of a given minimum magnitude that occurs in a given period following the onset of the conditioned stimulus and preceding the unconditioned stimulus. This frequency measure may be given in terms of percentage of subjects per trial exhibiting the response in the case of group data or frequency or percentage of responses per block of trials in the case of individual and group data.

Figure 6 presents frequency data for the conditioning of foreleg flection in a group of 30 dogs. The function, reported by Brogden (1949), was determined by means of the Vincent technique. In the experiment a fairly strong motivating condition was employed (the unconditioned stimulus was a strong shock to the forelimb), and each subject was trained until it reached a criterion of 100 per cent conditioning in a single period of 20 trials. It will be observed that the curve is S-shaped with the initial period of positive acceleration being somewhat shorter in length than the final negatively accelerated portion.

A considerable number of frequency curves of eyelid conditioning in human subjects are available in the literature, but for the most part they involve the averaging of groups of heterogeneous subjects and thus do not provide satisfactory data for our purpose. Typical examples of such data are presented in Figures 7 and 8 in which extremes of the types of curves that have been obtained are represented. Figure 7 shows curves of eyelid conditioning obtained by Passey (1948) for four groups of subjects that were trained under different intensities of air puff. All of the curves, it will be observed, are negatively accelerated from the beginning of training. Quite in contrast, the curves shown in Figure 8, taken from a study by Taylor (1951) in which the eyelid conditioning of anxious and non-anxious groups of subjects was compared, both exhibit an ini-

tial, positively accelerated phase. Indeed the lower curve for the
nonanxious group had not yet become negatively accelerated at
the point that conditioning was discontinued.

In order to obtain composite curves based on homogeneous
subjects the data of a number of studies of eyelid conditioning
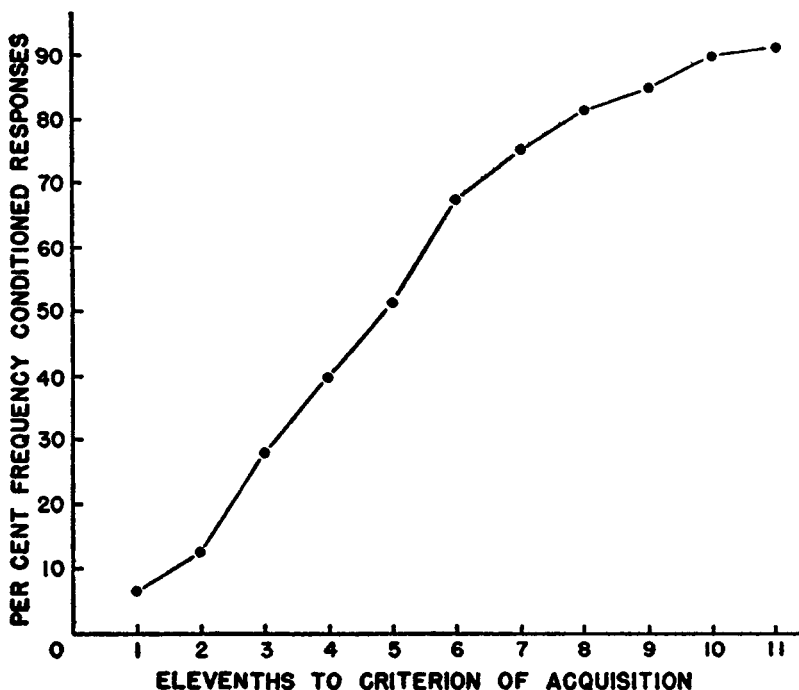
Fig. 6. Vincent curve of acquisition of a classical conditioned leg-flection
response in dogs. The points represent mean percentage frequency of con-
ditioned responses in the appropriate fraction of training for a group of 30 dogs.
Twenty trials per day were administered until the conditioning criterion of
100% was reached. A definite initial phase of positive acceleration may be
observed which is somewhat shorter in length than the final negatively ac-
celerated phase. (After Brogden, 1949.)

from our laboratory have been analyzed. Figure 9 presents the
curves of three subgroups of homogeneous subjects that were
run in previously reported experiments (Spence and Taylor,
1951). All subjects whose levels of response were highly similar
both for trials 1–40 and trials 41–80 were grouped together and

composite curves drawn. Thus the uppermost curve represents the composite curve for nine subjects, all of whom gave from 21 to 30 CR's in trials 1–40 and from 31 to 40 CR's in trials 41–80. The middle curve represents a composite function for seven subjects who gave from 5 to 13 CR's in trials 1–40 and
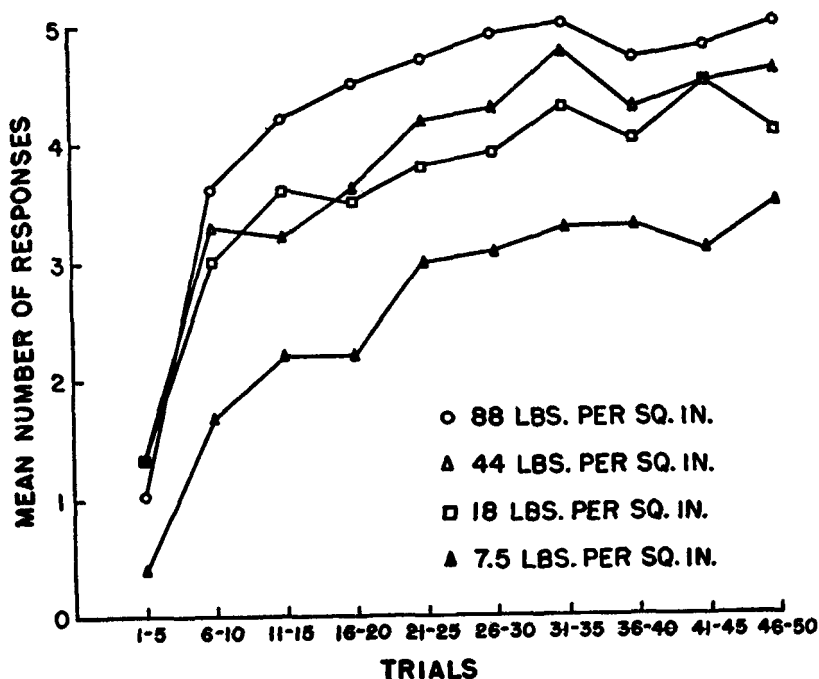
Fig. 7. Frequency curves of classical eyelid conditioning involving the averaging of groups of heterogeneous subjects trained under different intensities of air puff. (After Passey, 1948.)

from 17 to 30 responses in trials 41–80, while the lowest curve is for a group of seven subjects who gave from 0 to 6 and 6 to 17 on the criterion trials. The two lower curves, it will be noticed, exhibit a pronounced initial period of positive acceleration. The top curve, on the other hand, shows only a very brief period of positive acceleration, if any.

Examination of a fairly large number of such "homogeneous" frequency curves of eyelid conditioning obtained in our labora-

tory has shown that when the performance level of the subjects at the beginning of conditioning is relatively low and/or the rate of conditioning is slow a curve with an initial period of
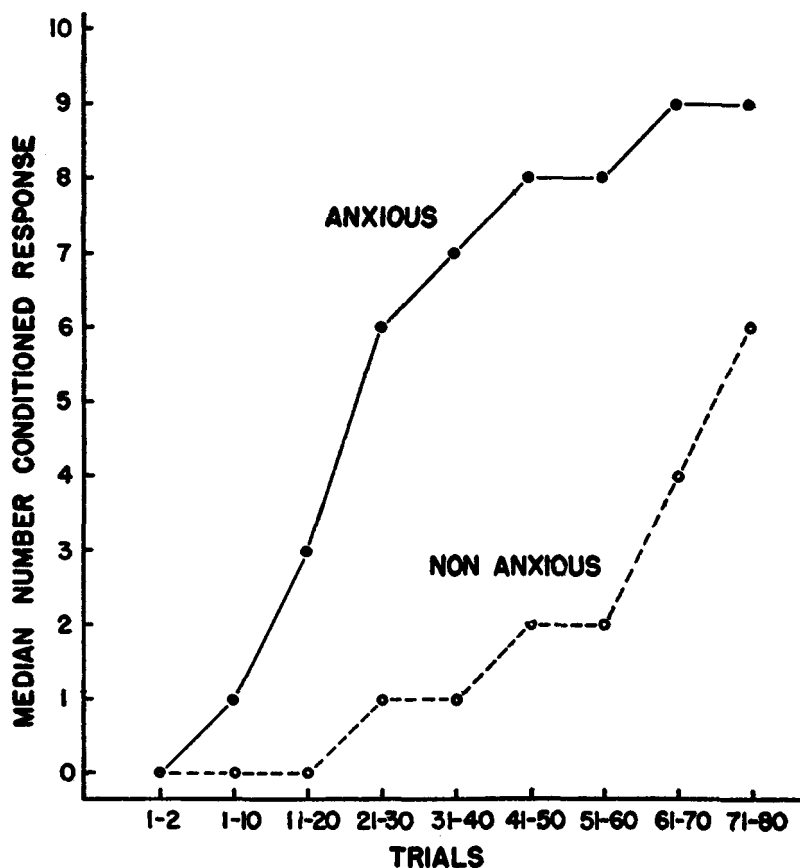
Fig. 8. Acquisition curves depicting the median number of classically conditioned eyelid responses for heterogeneous groups of anxious and nonanxious subjects. Note that both curves exhibit initial positive acceleration. (Taylor, 1951.)

positive acceleration is invariably obtained. On the other hand, if the performance level is relatively high at the start of conditioning and/or the rate of conditioning is high the curve is typically negatively accelerated throughout, especially if the

grouping of trials is coarse. The level of performance at which a curve becomes negatively accelerated tends to be at about the 20 to 30 per cent frequency level. The curves in the graph are an illustration of this, being negatively accelerated above this level of performance.
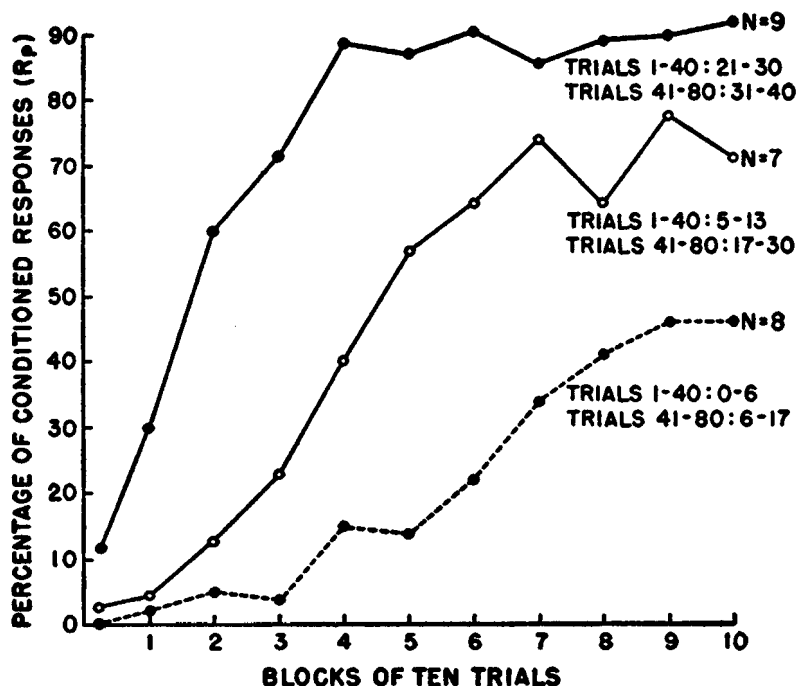
Fig. 9. Frequency curves of classical eyelid conditioning for three groups of homogeneous subjects. The uppermost curve is that for subjects selected on the basis of high conditioning performance both early and late in training, the middle curve is for subjects showing average performance in early and late training, and the lowest curve is for subjects showing poor performance in early and late training. The extent of the initial phase of positive acceleration apparently varies inversely with the general performance level. (Based on the data of Spence and Taylor, 1951.)

The composite curves in Figure 9, as we have described, were made up from subjects whose performances in the two criterion periods were highly similar. This method of selecting subjects, however, does not permit one to compare the curves of homo-
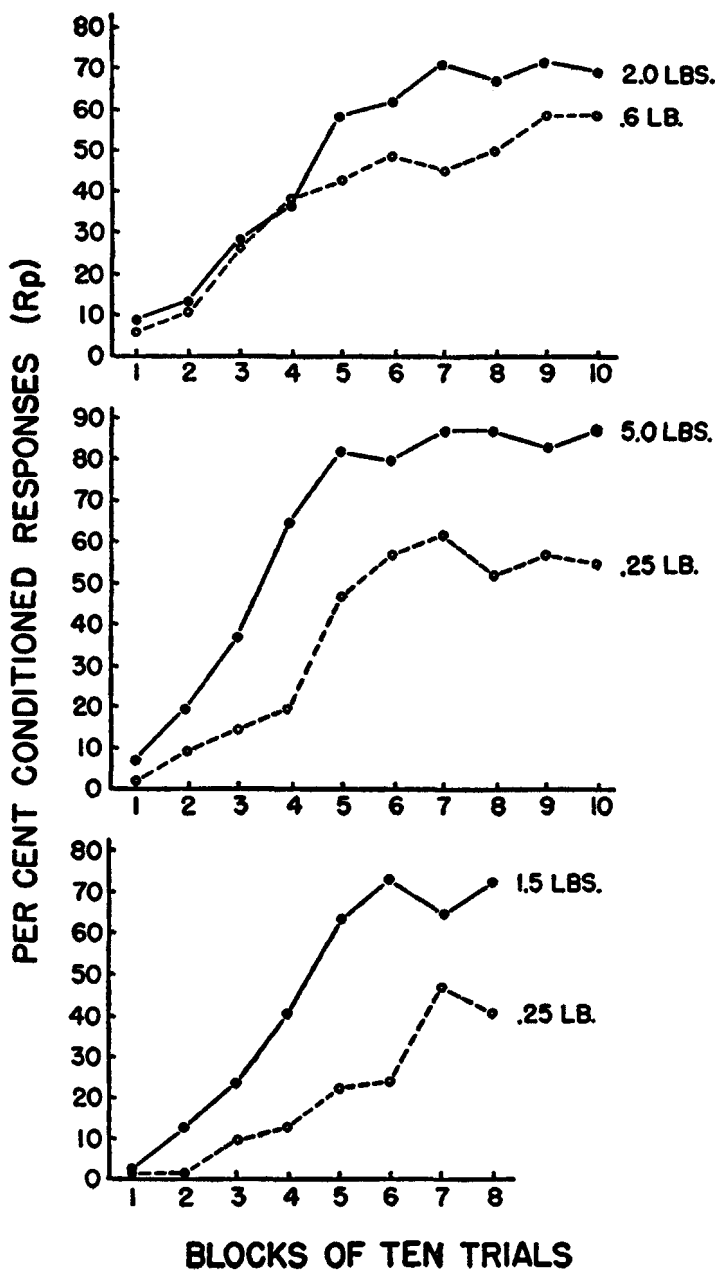
Fig. 10. Frequency curves of classical eyelid conditioning for groups of homogeneous subjects under different intensities of air puff. All curves are for subjects falling between the 40th and 60th percentiles in total number of conditioned responses given in trials 1–40. It may be observed that all the curves are S-shaped and that the extent of the initial phase of positive acceleration is inversely related to puff strength. (Based in part on the data of Spence and Taylor, 1951; and Spence, 1952.)

geneous subjects from one experimental condition to another. Such comparisons are only possible when the curves are based on subjects from comparable parts of their total distribution. In order to provide such comparable curves of more or less like subjects the following method was devised and used. All of the subjects run under a particular experimental condition were first rank ordered in terms of the total number of conditioned responses given in trials 1–40. Composite curves were then computed for the subjects falling between the 40th and 60th percentiles of the total distribution of subjects. Figure 10 presents such composite curves obtained from three separate eyelid conditioning investigations, all of which employed two different levels of intensity of the unconditioned stimulus. The top and middle set of curves are from studies (Spence and Taylor, 1951; Spence, 1952) that have already been published although the present data were not presented. The lowest pair of curves is from an unpublished study just completed in our laboratory. It will be observed that every one of the curves exhibits an initial phase of positive acceleration which is followed by a negatively accelerated approach to the asymptote. This latter phase is not so clearly present in the last pair of curves since only 80 conditioning trials were given. I should like to call attention also to another feature of the curves in each of these graphs, namely, that the curves for the different puff intensities show a gradual divergence with training. We shall consider the implications of this fact later in the chapter.

A final set of frequency data shown in Figure 11 presents individual curves of conditioned eyelid response obtained with monkeys by Hilgard and Marquis (1936). The number of trials involved in conditioning the monkeys was sufficiently large so that each point on the curve represents a large sample of trials (50), and hence the data are fairly smooth for individual subjects. As is readily apparent all three curves show the same type of S-shaped curve exhibited by the slower conditioning human subjects.

In summary these findings suggest the generalization that frequency curves of classical conditioning are S-shaped, *providing the complete course of the conditioning is represented in the curve.* Frequency curves that do not exhibit an initial, posi-
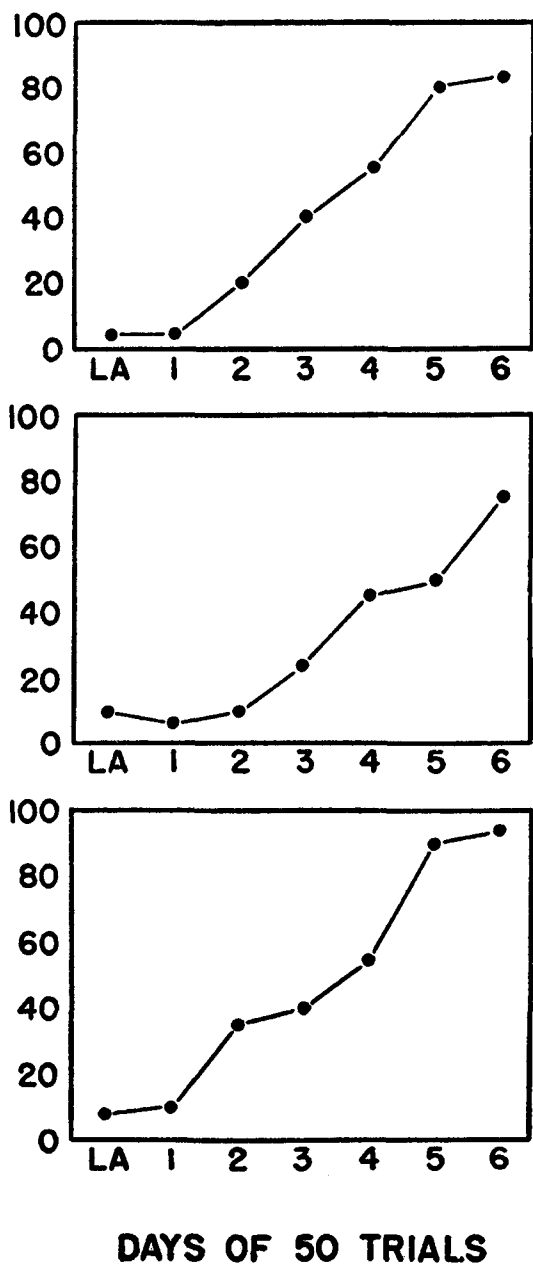
Fig. 11. Individual frequency curves of classically conditioned eyelid response for three monkeys. The light adaptation day (LA) yields a measure of the percentage of responses to the conditioned stimulus before conditioning. Since each point represents 50 conditioning trials it may be noted that the phase of initial positive acceleration is unusually extended. (After Hilgard and Marquis, 1936.)

tively accelerated phase do not do so either because they do not start at zero level of conditioning or because the conditioning is so rapid that the period of initial acceleration is too brief to be revealed except by very small groups or blocks of trials.[1]

Latency Curves: $R_t = f(N)$. A second measure of response strength in classical conditioning is latency. While a number of studies have employed this measure the findings with respect to latency have not been very consistent. An early study of Hilgard and Campbell (1936) presented data on the latency of anticipatory eyelid responses in human subjects which indicated that there is a progressive decrease in latency with successive conditioning trials. Analysis of latency measures of conditioned anticipatory responses in a number of our eyelid conditioning studies, however, has revealed very little consistency among the curves for either groups or individual subjects. Some evidence bearing on the variability in form and trend of such latency curves is revealed in Figure 12 which presents latency curves in blocks of 20 trials for the three groups of subjects who were highly homogeneous with respect to the frequency of their responses (see Figure 9). As is readily apparent the curves differ greatly from one another both in form and in the direction that the latency changes undergo.[2]

Just why subjects who show a high level of response (curve A) should provide a decreasing latency curve while subjects who show relatively few conditioned responses (curve C) should have a rising curve is not at all clear. Apparently the latency of the classical conditioned response is a complexly determined phenomenon concerning which we have as yet too little understanding. The temporal relation of the response to the reinforcing event is, it should be noted, very different in classical conditioning from what it is in the instrumental type. Thus speeding up of the response in instrumental conditioning brings

1. This finding of an initial period of positive acceleration in frequency curves of classical conditioning is not in accord with the statistical type of theory put forward by Estes (1950) and Estes and Burke (1953), which predicts a negatively accelerated (exponential) curve.

2. Analysis of latency data in terms of subjects who were homogeneous with respect to their latency measures gave similar results to those shown in Figure 12, some subjects showing increasing latencies with conditioning and others decreasing latencies.

the reinforcement sooner. This is not the case in classical conditioning. Indeed, responding too soon prior to the unconditioned stimulus may be regarded as less adaptive than responding just prior to it. Such early responses are not followed by immediate reinforcement and hence presumably are not reinforced or, at
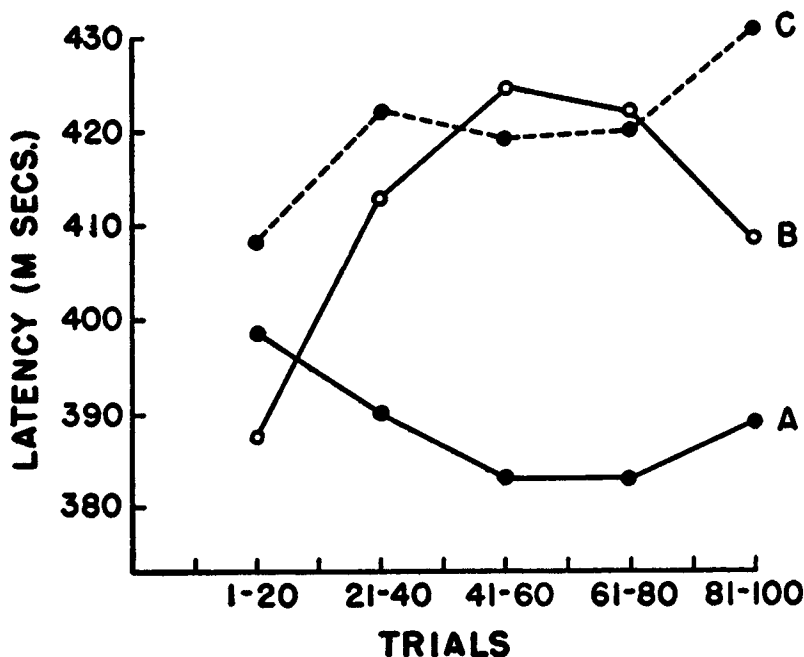
Fig. 12. Latency curves of classical eyelid conditioning for three groups of homogeneous subjects. A = the high-level conditioners of Fig. 9; B = the average-level conditioners; and C = the low-level conditioners. These curves display the typical irregularity of the latency measure in classical conditioning, perhaps indicating that a number of unknown factors are involved in its determination. (Based on the data of Spence and Taylor, 1951.)

least, not as much as responses that occur just prior to the unconditioned stimulus. The fact that the latency of a classical conditioned response is a function of the conditioned and unconditioned stimulus interval suggests the possibility that the latency of the response is part of what gets learned.

Amplitude Curves: $R_a = f(N)$. Amplitude or magnitude of a response is a third measure of response strength that has been

employed in a number of conditioning studies, amplitude being measured in terms of such scales as length, electric resistance, volume, and so on. Such measures have been used primarily in the case of responses under the control of the autonomic nervous system. Thus in the classical Pavlovian experiments the strength of the conditioned response was measured in terms of the amount of saliva secreted in a given interval of time following the onset of the conditioned stimulus. Unfortunately the Russians did not go in strongly for such quantitative data, and we have very few reported curves of salivary conditioning from their studies. Hull (1934c) presented a modified Vincent amplitude curve of salivary conditioning based on four dogs run by Kleitman and Crisler (1927). This curve, reproduced in Figure 13, shows a very definite and rather prolonged period of positive acceleration. Presumably if conditioning had been continued for a longer period the curve would have eventually become horizontal and the complete curve would have been S-shaped.

Amplitude measures have also been employed in connection with the galvanic skin reaction (GSR) and the pupillary response. Again, however, there is very little satisfactory data available on the form of the conditioning curve for these responses. A somewhat unusual type of conditioning curve in that the successive points on the function represent the performance of different groups of subjects rather than the same subjects has been reported for the galvanic response in human subjects by Hovland (1937). Shown in Figure 14, this curve was obtained by measuring the response of comparable groups of subjects on a test trial in terms of the mean magnitude of deflection of a galvanometer after different numbers of reinforced trials. The different groups of subjects were equated on the basis of their responses to the electric shock (UCS) and to the tonal stimulus (CS) prior to conditioning. The value for zero reinforcements represents the mean of the last two responses prior to conditioning of all subjects to the conditioned stimulus. As may be seen the empirical points conform very closely to the negatively accelerated exponential function fitted to them by Hull (1943, p. 103).

So far in our consideration of measures of amplitude of response we have considered only effector systems that are under

the control of the autonomic nervous system. In his *Principles of Behavior* Hull limited his treatment of response amplitude to such autonomically controlled responses, for, as he showed, in the case of striated muscle responses subjects may be trained to respond with a particular amplitude. As an instance of this, Hull (1943, p. 304) reported an experiment in which rats were
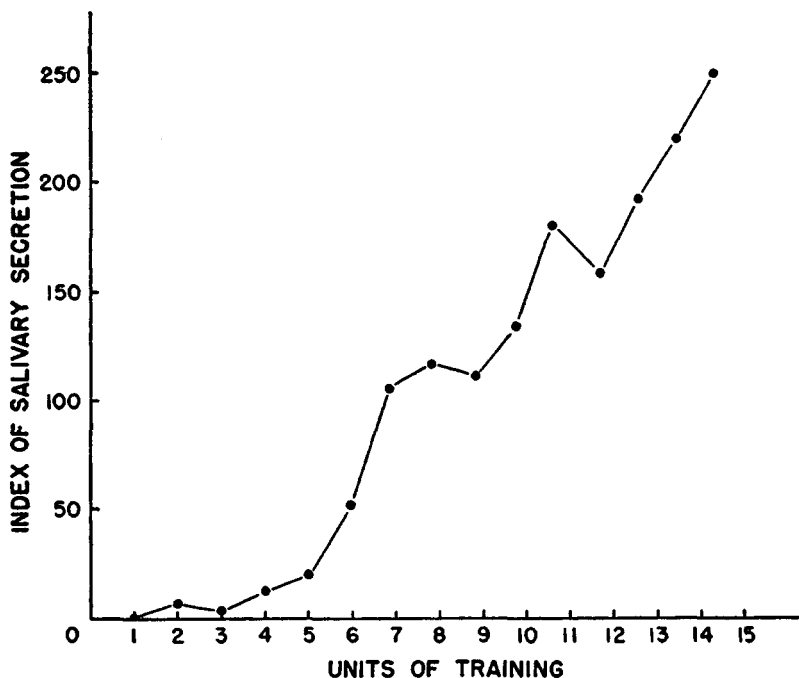
Fig. 13. A modified Vincent amplitude curve of classical salivary conditioning based on the performance of four dogs. (The Vincent curve data were derived by Hull, 1934, from unpublished data provided by Kleitman and Crisler, 1927.)

trained to depress a lever with different pressures depending upon the force required to deliver the reinforcing stimulus. While this complication holds in the case of such instrumental responses it need not necessarily exist in classical defense conditioning of the type involved in limb flection or eyelid closure. In the latter instances the response does not involve any action that alters the environment but merely moves some part or organ of

the body and thus changes the relation of the organism to the environment.

A number of investigators (e.g., Hilgard and Marquis, 1935; Hilgard and Campbell, 1936; Humphreys, 1940) have reported eyelid conditioning curves in which the response measure was
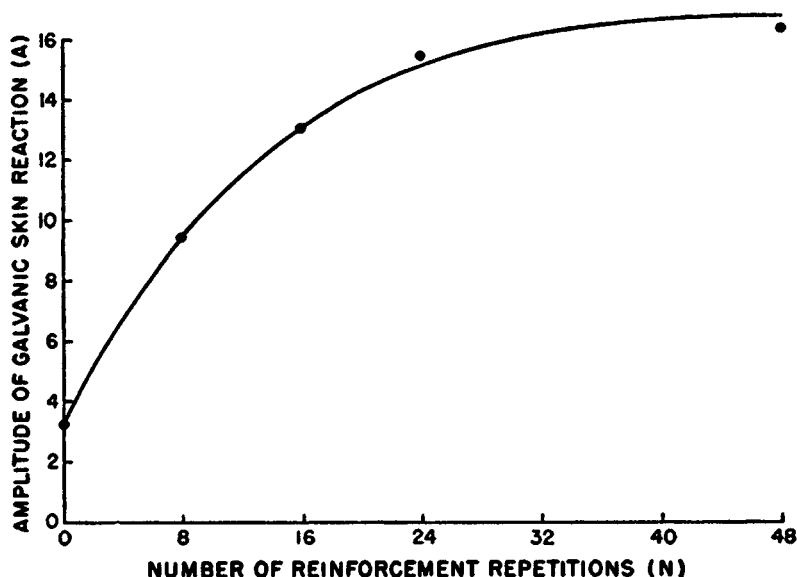
HULL FIG. 21

Fig. 14. An amplitude curve of the classically conditioned galvanic skin response plotted in terms of the amplitude in millimeters of the first galvanic skin reaction evoked by the conditioned stimulus. The points represent the mean test performance of different but comparable groups of subjects after varying numbers of reinforced acquisition trials. The value for zero reinforcements is the mean of the last two responses prior to conditioning of all subjects to the conditioned stimulus. The data are fitted by the exponential function, $A = 14.1(1 - 10^{-.033N}) + 3.1$. (Hull, 1943, from data published by Hovland, 1937.)

the extent of the eyelid closure. Attention should be called to the fact, however, that the majority of these studies have included zero amplitude responses (absence of an anticipatory response) in the calculation of the average size of the response. This measure, which has been termed *magnitude* by Humphreys (1943), is essentially a weighted frequency measure, and the

evidence indicates, as might be expected, that curves employing this measure tend to follow closely the form of frequency curves. (See Hilgard and Campbell, 1936, p. 231, Fig. 2.)

In contrast to this magnitude measure of eyelid conditioning, a measure designated by Humphreys as *amplitude* is determined by including in the calculation of the average only those instances in which an anticipatory conditioned response occurs. That is, zero values are not included in the measure thus providing the average amplitude of those responses that do occur.
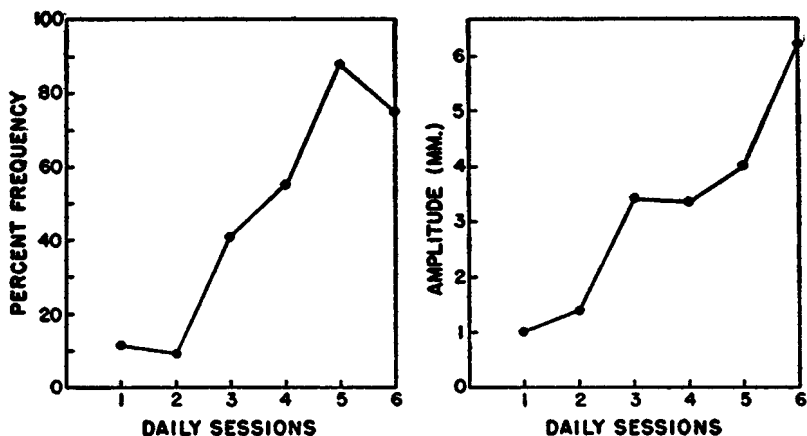
Fig. 15. Individual frequency (left) and amplitude (right) curves of classical eyelid conditioning for the same human subject. The amplitude curve is based on data from only those trials in which an anticipatory conditioned response occurs; i.e., zero values are not included in the calculation of the average. It may be seen that both curves exhibit prolonged phases of positive acceleration. (After Hilgard, 1931.)

In one of the earliest studies of eyelid conditioning Hilgard (1931) obtained such an amplitude curve for a single subject. The conditioning procedure employed a light as the conditioned stimulus and a sound as the unconditioned stimulus. The subject was given from 31 to 35 trials on each of the first five days of training and 12 on the sixth day. Plotting the mean amplitude of the conditioned anticipatory responses by days gave the curve shown in the right half of Figure 15. The left half presents the frequency curve for the same subject. As may be seen

both curves suggest a definite initial period of positive accelera-
tion. Presumably the amplitude curve would have eventually be-
come negatively accelerated with further training.

There are a number of difficulties involved in the measure-
ment of a conditioned eyelid response which have discouraged
us from employing either the amplitude or magnitude measure.
One of the major difficulties arises from the fact that in many
instances the anticipatory eyelid closure begins just prior to the
onset of the unconditioned stimulus and thus blends in with the
unconditioned response. About the only way in which the ex-
tent of closure of such late occurring responses can be measured
is to have test trials in which the unconditioned stimulus is
omitted. Unfortunately such omissions merely introduce further
complicating factors. Still another difficulty is the small number
of responses that are available for measurement during the
early stage of training. Such small samples provide extremely
unstable measures of central tendency with the result that
curves of amplitude are typically highly irregular.

In summary, the available evidence as to the form taken by
classical conditioning curves employing amplitude measures of
response strength is somewhat conflicting. Both negatively ac-
celerated and S-shaped curves have been obtained. However,
the fact that curves for individual subjects and Vincentized
curves for groups of subjects have tended to be S-shaped sug-
gests that more adequate data may reveal this type of relation-
ship to hold between measures of response amplitude and the
number of conditioning trials. Some further experimental evi-
dence on this function from our own studies with the eyelid
response will be presented in the next chapter in connection with
our theoretical treatment of conditioning curves.

CURVES OF INSTRUMENTAL CONDITIONING

Frequency Curves: $R_p = f(N)$. Turning next to the con-
sideration of acquisition curves in instrumental conditioning
the presentation will again necessarily be brief, for while there
are a great many curves available in the experimental literature
the majority are for groups and hence are of little use to the
theorist for the reasons already discussed. The most frequently

used measures of response strength in instrumental conditioning are latency and duration of the response, both of which may be translated into speed measures by taking their reciprocals. Other measures that have been employed are resistance to extinction and rate of responding such as used in the free-responding Skinner box. Surprisingly enough, as far as I am aware, no one has used the frequency measure to describe the course of learning in instrumental conditioning.

A frequency measure is, nevertheless, easily derivable from time data in this kind of situation, and I shall begin with some examples of this type of curve recently obtained in our laboratory. By selecting an arbitrary minimum latency (or duration) to define the response, a frequency curve may be obtained by counting in any block of trials the number of responses of shorter latency (or duration) than this criterion value. Furthermore, by selecting different critical time values one may obtain a family of frequency curves from the same experimental data. Figure 16 presents such frequency curves as they were determined from the data of an experiment conducted by Ramond (1954a).[3] The experimental situation was a simple runway in which time was measured from the moment a glass door leading from the starting box was raised until the subjects (hooded rats) ran to and touched a projecting bar at a distance of 12 inches. Since the bar was placed immediately above (⅛ in.) a food box from which the subjects had been trained to obtain food the subject did not have to learn where to go to find the food.

The data shown here were obtained from a group of 10 subjects run under 22 hours of food deprivation and with a reinforcement delay of one second. Frequency of responses for successive 10 trial blocks was determined for each of the 10 subjects. Two different criterial values, 1.7 and 2.5 seconds, were employed. The frequency measure was computed simply by counting in each successive block of 10 trials the number of responses that were less than these criterial durations. Like or homogeneous subgroups were then selected by grouping the five subjects giving the highest frequency in trials 1–50

3. The writer is indebted to Charles Ramond who kindly made available the latency data from which these frequency curves were determined.

into one group and those giving the lowest frequency in the other. The broken lines represent the curves for the fast performers, while the curves drawn with solid lines are for the slow ones. As may be seen, the general form of the curves is S-
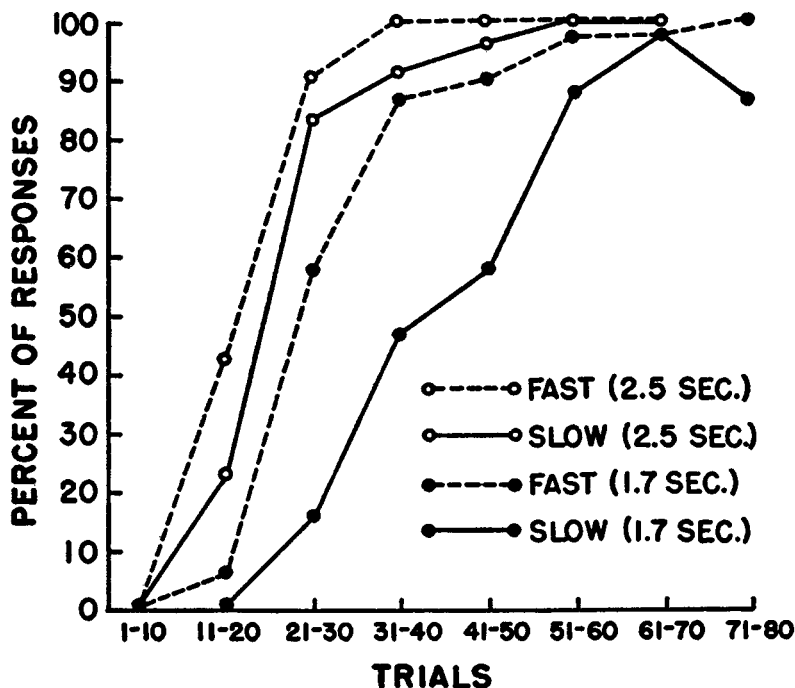
Fig. 16. Frequency curves of acquisition of an instrumental bar-touching response. The curves were derived from latency data by computing the percentage of responses in each block of trials that are shorter in duration than some arbitrarily selected criterion latency. The performance curves of two homogeneous groups of rats (fast and slow performers, $N = 5$ per group) under relatively lenient (2.5 secs.) and severe (1.7 sec.) latency criteria are shown, all exhibiting an initial phase of positive acceleration. (Based on the data of Ramond, 1954a.)

shaped. Similar curves were obtained for three other groups run by Ramond under less favorable drive and reinforcement conditions.

Latency Curves: $R_t = f(N)$ *and Speed Curves:* $R_{1/t} = f(N)$. Turning next to instrumental conditioning curves in-

volving measurement of the temporal characteristics of response we find two main types. In one set the latency or duration of the response or sequence of responses is determined; in the other the time measure is translated into a speed measure by calculating its reciprocal. A number of somewhat different methods of measurement have been employed. Thus in the simple runway time may be measured from the raising of the door of the starting box to a point just within the alley (a measure known as response latency or action evocation time). Another widely used measure involves the time of running in the alley from some point *after* the subject has started to some further point in the alley. This measure is known as running time. Or the response time may be measured from the raising of the door to a point at the further end of the alley. This measure involves action evocation time plus running time. Reciprocals of these time measures are referred to respectively as speed or velocity of action evocation, running speed, and a combination of the two.

Most of the published curves of instrumental learning are in terms of one or other of the *time* measures. Typically these have shown a negatively accelerated *decreasing* function to which a variety of equations have been fitted. Translation of the data of these studies into speed measures and examination of the resultant curves, along with other speed curves available in the literature, have revealed a variety of different shaped functions. While not particularly significant because of being based on heterogeneous groups, nevertheless it is perhaps worth noting that the most prevalent type was the S-shaped curve. The second most frequently appearing type was more or less linear in its early phase, becoming negatively accelerated as the asymptote was approached. In a number of instances a curve negatively accelerated throughout its course was obtained.

Presented in Figure 17 are *speed curves* (speed of evocation plus running speed) for successive blocks of five trials for two groups of like subjects run in a simple instrumental conditioning setup. The upper curve is for the same five slow learning subjects whose frequency data were presented in the previous graph. These subjects were trained under 22 hours of food deprivation and with a one-second delay of reward following

contact with the bar. The lower curve is for a second group of five homogeneous subjects run in the same experiment under less favorable conditions, the deprivation period being only four hours and the delay-of-reward period five seconds. Not only were the two groups of subjects homogeneous but each also represented the five slowest running subjects out of a
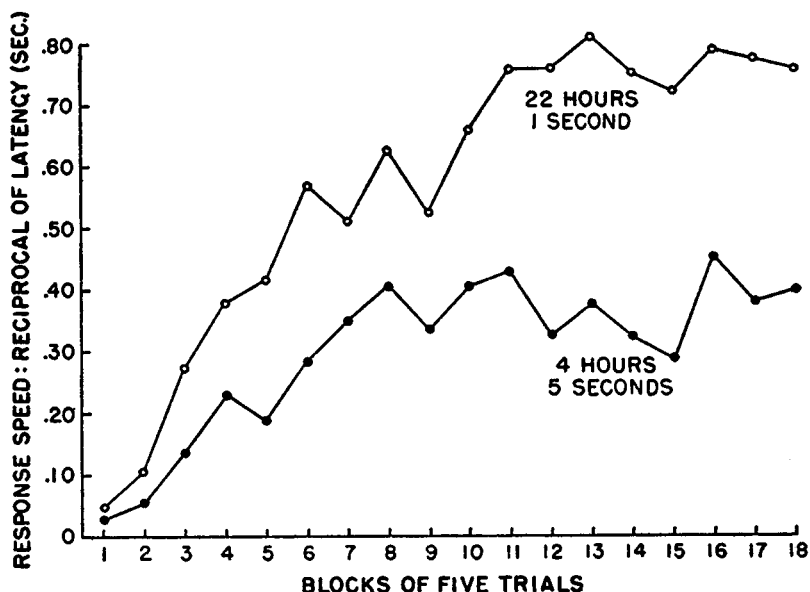
Fig. 17. Speed curves of acquisition of an instrumental bar-touching response. These curves present the data of two homogeneous groups of slow running rats under favorable (22 hrs.: 1 sec.) and unfavorable (4 hrs.: 5 secs.) time of deprivation-delay of reward conditions. Both curves exhibit a brief initial phase of positive acceleration. (Based on the data of Ramond, 1954a.)

total of 10 subjects in each group. The difference in the levels of the curves is a reflection, then, of the difference in effectiveness of the motivational-reward conditions. Both curves, it will be observed, are S-shaped. Again, an examination of a considerable amount of such data from the Iowa laboratory has shown that curves involving *evocation speed* based on such homogeneous populations of subjects have invariably been S-shaped. On the other hand, *speed-of-running* curves have tended

to be negatively accelerated in form as well as S-shaped in some instances. Presentation of this type of curve will be deferred until the next chapter in connection with the implications for it of our theoretical model.

Curves Based on Resistance to Extinction: $R_n = f(N)$. Although measurement of response strength in terms of resistance of the response to extinction does not provide a conditioning curve in the usual sense of the term, i.e., a series of performance measures for a single group of subjects, nevertheless, this measure may be used to obtain a function relating response strength to the number of original reinforcements. The procedure, like that employed by Hovland in his GSR study, involves using different groups of subjects which are first given different numbers of conditioning trials. The average strength of response of the differentially conditioned groups is then measured by determining the mean numbers of times the subjects in each group continue to respond in a series of extinction trials in which reinforcement is no longer present. Different criteria of extinction are employed, such as a five-minute period without a response in the free-responding Skinner-box apparatus or three successive failures of occurrence of the response to the conditioned stimulus in the discrete trial type of instrumental conditioning.

The most extensive data of this type are from two studies conducted in Hull's laboratory (Perin, 1942; Williams, 1938). Curves showing the relation of the number of extinction responses (R_n) to the number of original reinforcements (N) at two levels of drive in a Skinner-box experiment are shown in Figure 18. The upper curve was determined at 22 hours of food deprivation by Williams, while the lower curve for three hours' deprivation was obtained by Perin. As may be seen, the negatively accelerated exponential functions drawn through the empirical points fit both sets of experimental data quite well.

This ends our examination of the experimental findings with respect to these simple conditioning curves. As we have seen, the most general statement that can be made concerning them is that if increasing response strength is represented on the ordinate the functions for the different response measures in the several types of situations rise in a monotonic function to a

limiting value or asymptote. Depending upon the measure used and the conditions of the experiment, such as the strength of motivation, the curves vary in form from being negatively accelerated throughout their course to functions that exhibit a pronounced initial period of positive acceleration followed
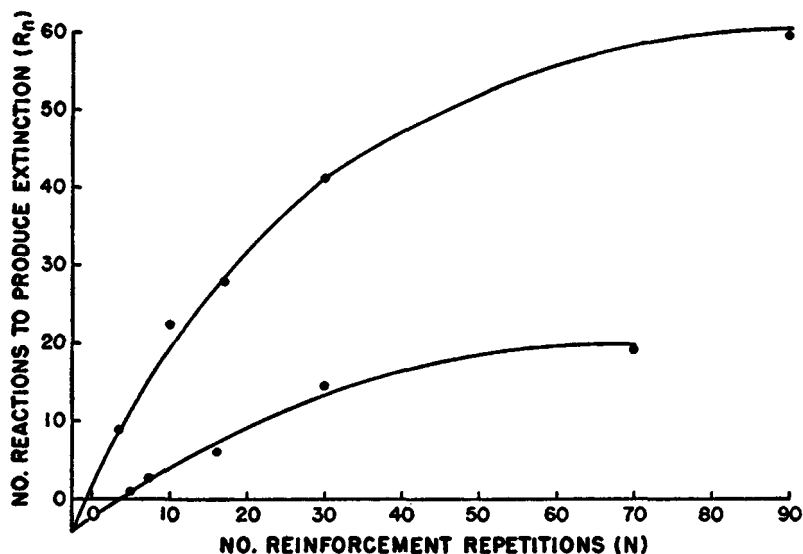
Fig. 18. Instrumental conditioning of a bar-pressing response in terms of resistance to experimental extinction. The points represent the mean test performance of different groups of rats after varying numbers of reinforced acquisition trials. The data of groups extinguished under 22 hours of food deprivation are presented on the upper curve, that for groups under three hours of deprivation on the lower curve. Note that both curves exhibit negative acceleration throughout their entire courses. (Hull, 1943, from data published by Williams, 1938, and Perin, 1942.)

by a final, negatively accelerated approach to the asymptote. In between these two extremes are found curves that appear initially to be more or less linear in form and which finally become negatively accelerated as the asymptote of performance is reached. We shall return to these data in the next chapter when we come to the formulation of our specific theoretical structure. Indeed the latter will be fashioned in such a manner

as to derive the various forms the curves for the several response measures take under the different experimental conditions.

CONDITIONING PERFORMANCE AS JOINT FUNCTION OF TRAINING AND MOTIVATION

In concluding this chapter I should like to consider briefly one further set of experimental data which we shall need in connection with the initial formulation of our theoretical model. In several instances, it will be recalled, conditioning curves for different levels of motivation were presented, and it was found that performance level was higher (stronger) the higher the motivational level. While we shall leave the specific problems as to how response strength varies with the conditions of motivation until a later part of the book I should like to present the experimental findings with respect to how the learning variable (N) acts jointly or combines with such experimental motivational variables as time of deprivation of food (T_d) or intensity of the noxious unconditioned stimulus (S_u) to determine response strength.

If we look again at Figure 18 which presented the Perin-Williams findings on resistance to extinction we see that it contains data relevant to this problem. The curve for the 22-hour hunger group rises to a higher level than that for the three-hour group. Furthermore, the fact that the curves for the two hunger drive levels diverge from one another with increasing numbers of reinforced trials suggests that the two experimental variables, N and T_d, combine in a multiplicative manner rather than in an additive fashion. Or, to put it another way, these data suggest that the joint empirical law relating R with N and T_d is of the type shown in the first of the following equations and not that shown in the second:

$$(1) \quad R = (fN) \times (fT_d)$$
$$(2) \quad R = (fN) + (fT_d)$$

If the law were a simple additive one as represented in the second equation the two curves would have been parallel throughout their course.

Examination of some of the other earlier graphs (Figures 7, 10, 17) in which there were curves at different levels of motivation will reveal that all exhibited this same divergence. Some additional evidence from recent studies conducted in our laboratory are presented in Figures 19 and 20. The first of these
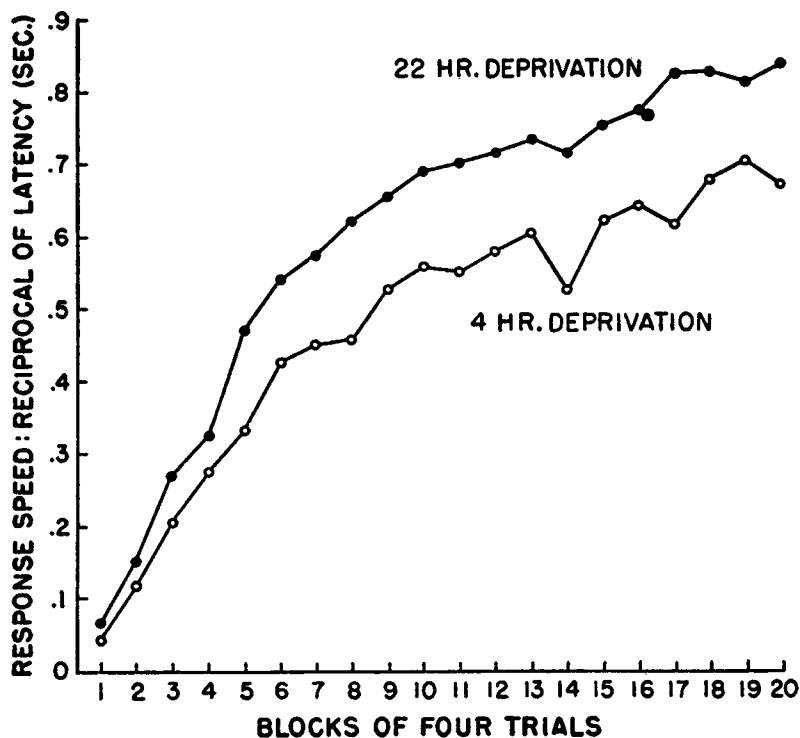
Fig. 19. Speed curves of acquisition of an instrumental bar-touching response at two levels of hunger. The increasing difference between the motivational groups with training trials suggests that T_D and N combine in some nonadditive fashion to determine the performance level. (After Ramond, 1954a.)

two graphs shows speed-of-response curves obtained by Ramond (1954a) for two groups of subjects, one of which was under 22 hours of food deprivation and the other four hours' deprivation. The gradual divergence of the curves is clearly evident.

Data from an instrumental escape conditioning situation which required the animals to run down a four-foot alley, the

floor of which contained a charged electric grid, to escape from shock are presented next in Figure 20. The curves, obtained in a master's thesis by Ketchel (1955), represent running speed as a function of training trials under two levels of shock intensity, 11.6 volts and 36 volts. The two curves clearly show an increasing divergence, although the data suggest that the
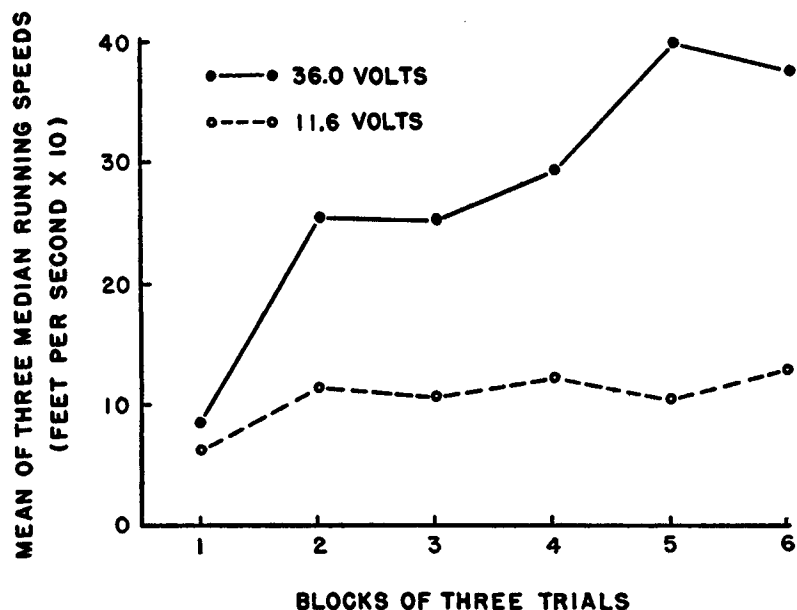
Fig. 20. Speed curves of acquisition of an instrumental running-to-escape-shock response at two intensities of shock. The values on the ordinate were obtained by computing the median speed for each subject for successive blocks of three trials and then averaging these medians over groups. (After Ketchel, 1955.)

weak shock did not provide for very much improvement in performance.

On the other hand two experimental studies (Amsel, 1950; Campbell and Kraeling, 1953) in which this instrumental escape type of situation was used failed to find such diverging curves under different levels of shock intensity. Thus speed-of-running curves based on the data reported by Amsel gave parallel curves for two shock intensities, while, as may be seen from Figure
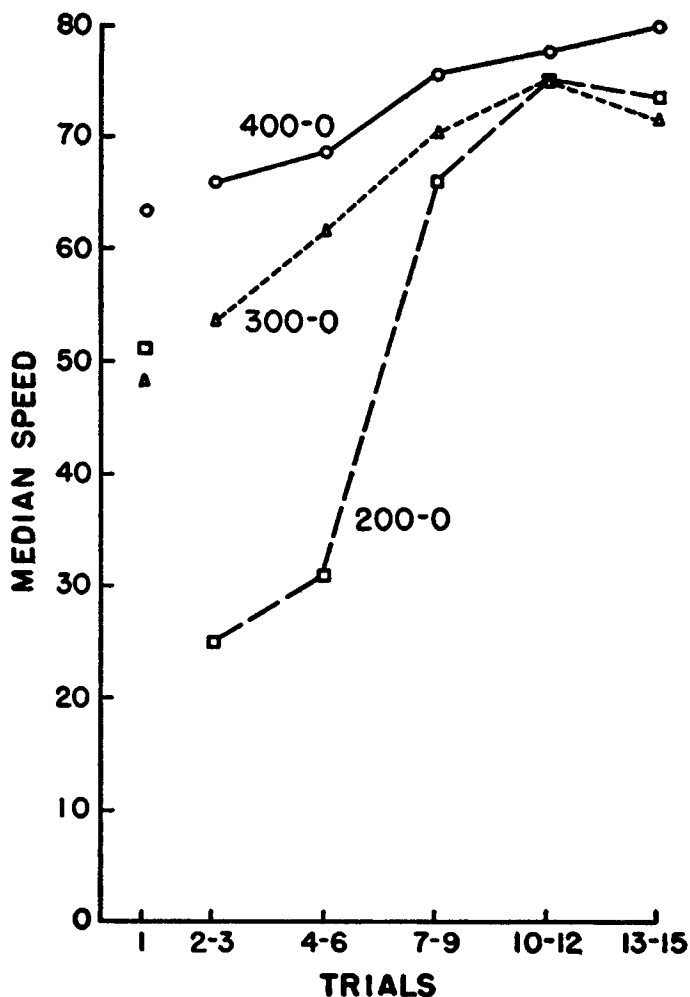
Fig. 21. Speed curves of acquisition of an instrumental escape response for groups running from various levels of shock to zero volts in the goal box. Ordinal values are medians of individual trial medians, except that trial 1 medians are plotted separately to show subjects' unconditioned rates of running at various levels of initial shock. (Campbell and Kraeling, 1953.)

21, the results from Campbell and Kraeling deviated even more from the diverging pattern, the curves for different intensities tending to converge toward a common asymptote.

Nevertheless, in view of the fact that the bulk of the experimental evidence has indicated that conditioning curves under different motivational levels tend to diverge we shall tentatively assume that the two kinds of motivational variables (T_d and S_u) act jointly with the learning variable (N) in a multiplicative relation to determine response strength. Just what limitations may have to be placed upon the range of motivational levels within which this law will hold remains to be determined. For the present we shall assume that it holds up to hunger and thirst levels produced by 24 hours' deprivation and in the low to medium range of intensities of noxious stimulation. Much experimental work remains to be done along the lines of determining the limits within which such laws hold.

4. Theoretical Model Based on Conditioning Curves

IN THE LAST CHAPTER it was stated that the initial objective of the type of theorizing in which Hull and others of his group have been engaged is the formulation of a set or system of interrelated abstract concepts, which in combination with the different initial and boundary conditions of the particular experimental situation would provide for the derivation of the various phenomena of conditioning. The main task of the present chapter will be to present and consider some of the problems involved in formulating such a theoretical model for the acquisition laws of conditioning described previously. Before getting into the details of the theory, however, I should like to make some preliminary remarks of a general methodological nature concerning the structure of this type of theory. I shall begin with a series of comments that are primarily aimed at eliminating some erroneous notions.

The first is that the theory is of the type that mathematicians call an axiomatic system. This misconception undoubtedly arose from the fact that in his miniature-system phase in the late thirties Hull employed much of the trappings of such formal language systems, e.g., undefined terms, postulates, theorems, etc. Such axiomatic system types of scientific theories, of which there have been only a few instances even in physics, begin with a set of undefined terms between which postulated relations (called implicit definitions) are stated. By means of logical operations theorems are then deduced. This purely formal, nonempirical system is then "interpreted" by coordinating a basic class of empirical concepts (experimental variables) either to the original terms of the system or to compound terms introduced from them by means of explicit definitions.

Whereas the concepts and postulated relations of such axiomatic theories are initially quite independent of the experi-

mental data for which they are proposed, the concepts of our type of theory have their origin in the findings of learning experiments themselves. They are actually formulated on the basis of the observed laws relating the experimental variables in these conditioning experiments, and they are deliberately fashioned so as to be able to derive these laws. In its initial formulation, then, this type of theorizing is a purely ad hoc affair. Once formulated on the basis of one set of learning data, however, the theory is subsequently applied to new data either from the same type of situation or from other, more complex learning situations. That is, rational equations representative of empirical relations to be expected in the new data are deduced on the basis of the original theory. It is by virtue of this application to new, different learning situations that the theory ceases to be circular and ad hoc.

At the opposite pole from this first misconception of our theorizing is a second, equally erroneous notion that this theoretical approach is seeking a *single* general equation with a sufficient number and kinds of parameters as to permit the fitting of any and all conditioning curves, whatever their form. Actually the fitting of such an empirical equation to the different curve forms would have no particular scientific worth unless the experimental variables determining the values of the parameters of the equation could be specified. One does not accomplish this merely by "finding" such a mathematical equation. The main task at this stage of development of our knowledge, that of showing how the different experimental conditions interact or combine to produce the different forms taken by the learning curves, would still remain. Quite in contrast to this purely curve-fitting approach our theorizing has attempted to formulate the manner in which the different experimental variables combine to determine the several response variables. This has involved the introduction of different combinations of constructs and postulated interrelations among them, from which the variations in conditioning curves that occur under the different experimental and measurement conditions may be derived.

Common to all of these combinations as far as learning or acquisition curves are concerned is a single construct repre-

senting the operation of reinforcing a response. Thus in the case of Hull's theory the concept of associative or habit strength, defined as a negatively accelerated, exponential function of the number of reinforced trials, was introduced. In turn, each of the different response measures was related to this learning or habit factor by an appropriately assumed relation. Furthermore, specification was also made as to the manner in which motivational and incentive factors interact with this learning factor to determine response strength. The latter assumptions have led in Hull's theorizing to the introduction of such concepts as generalized drive strength (D), incentive motivation (K), and excitatory potential (E).

If we may neglect for the moment all of the theoretical machinery but the learning factor (H), the nature of this type of theorizing may perhaps be made clearer by the graphs in Figure 22. The upper three graphs, A, B, and C, represent in geometrical form the assumptions that Hull made in connection with instrumental conditioning curves involving the response measure, resistance to extinction (R_n). The theory began with the exponential assumption concerning the development of H with successive reinforcements of the response (N). R_n was then assumed to be related to H in a linear manner as represented by the second graph. These two assumptions when combined produce the exponential relation shown in the third graph. Hull chose this combination of assumptions because the implied relation of R_n to N agreed with the available experimental data. Now it would have been possible, of course, to have assumed that the relations in graphs A and B were the reverse, i.e., that H developed as a linear function of N and that R_n was related to H by the negatively accelerated exponential function. Hull's reason for choosing the former alternative was based upon the available empirical data with respect to another set of acquisition curves, namely those involving the frequency measure in classical conditioning. The little evidence available at the time suggested that such frequency curves had a distorted ogival form of the kind shown in graph F at the lower right-hand corner of the figure. In this curve the initial period of positive acceleration is much shorter than the ensuing negatively accelerated phase. Such a distorted function followed from the

combination of a negatively accelerated habit-growth function and the type of relation between response frequency (R_p) and habit (H) shown in graph E. The latter relation was implicit in Hull's concept of oscillatory inhibition. The implication of a linear habit-growth function, when combined with the concept of oscillatory inhibition, on the other hand, was that such
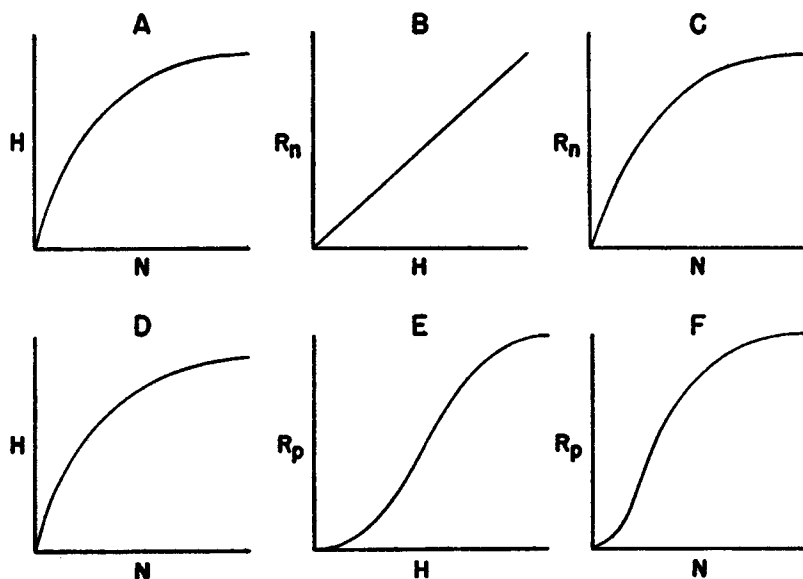
Fig. 22. Graphic representation of Hull's assumptions relating habit strength (H) to the number of training trials (N) on the antecedent side and to two response measures, resistance to extinction (R_n) and response probability (R_p), on the consequent side. Graphs C and F depict the two empirically ascertainable relationships, $R_n = f(N)$ and $R_p = f(N)$. If an exponential relationship between H and N is then assumed, as shown in graphs A and D, the relationships shown in B and E are necessarily fixed. See text for further explanation.

frequency conditioning curves would be symmetrical and not distorted.

The diagram does not, of course, tell the whole story, for empirical curves involving other response measures were also involved in the initial decisions concerning the assumptions that were made. However, my purpose here has not been to present the theory in complete form but merely to explicate this

preliminary consideration of the manner in which such theorizing proceeds. I should like to add that of necessity there is a considerable element of arbitrariness in the selection of the various functions for there are often a number of different combinations of assumptions that will work for the available data, particularly when the number of different empirical functions is rather small. In such circumstances the theorist simply must take the bull by the horns and go ahead with a particular selection of assumptions. Often there are related theoretical treatments that influence him in his decision. As an example of this latter sort of thing I suspect that my own preference for the exponential rather than the linear assumption concerning the increase of habit strength with training is based, in some small part at least, on the knowledge that Rashevsky (1938), a mathematical biologist, has derived such a function to describe the growth of the hypothetical stimulus-response associative strength from a mathematical-neurological model. The fact that Estes (1950) has likewise arrived at such an exponential function describing the increasing strength between a stimulus complex and its associated response from certain statistical assumptions has also undoubtedly been a factor.

THEORETICAL MODEL: RELATION OF INTERVENING TO INDEPENDENT EXPERIMENTAL VARIABLES

With this preview of how this type of theorizing proceeds let us turn now to the portion of our detailed theory that is most relevant to conditioning curves. Figure 23 shows the experimental variables and theoretical concepts that will be employed. For the present we shall not attempt to treat the variables specific to the reinforcing event but will assume that the conditions of reinforcement, for example the magnitude and delay of the reinforcer, are adequate for learning to occur. In the next chapter we shall take up the role of these variables, particularly as they affect instrumental conditioning. Likewise we shall not consider work or fatigue factors but will assume that we are dealing with distributed practice conditions in which such factors play a negligible role, if any.

At the top of the graph are shown the experimental variables

in terms of which the theoretical concepts or intervening variables to be employed are introduced or specified. Three of the empirical response measures employed in simple conditioning experiments, probability or frequency of response (R_p), ampli-
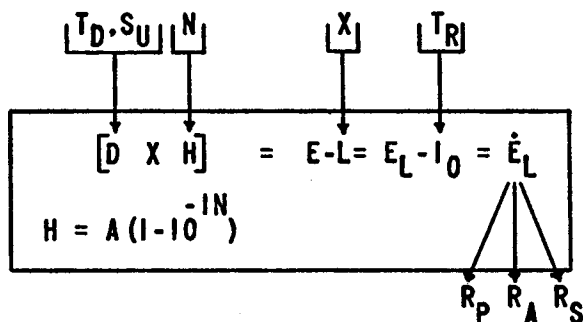
Fig. 23. Diagram representing portion of theoretical model most relevant to conditioning curves. The independent (experimental) variables (e.g., T_d) are shown above the rectangle and the dependent (response) variables (e.g., R_p) below. Inside the rectangle are depicted the theoretical (intervening) variables and their interrelationships. The arrows leading into the rectangle represent the relationships between the experimental and intervening variables; the arrows leading out of the rectangle represent the relationships between the final intervening variable, $\dot{E}_L$, and the response measures. In this diagram $T_d =$ time of deprivation; $S_u =$ intensity of noxious stimulation; $N =$ number of training trials; $X =$ nature of the experimental conditions; $T_r =$ instant in time; $D =$ general drive level; $H =$ habit strength; $E =$ excitatory potential; $E_L =$ superthreshold excitatory potential; $\dot{E}_L =$ momentary excitatory potential; $I_o =$ oscillatory inhibition; $R_p =$ response probability; $R_a =$ response amplitude; $R_s =$ response speed; $A =$ limit of habit growth; $i =$ parameter determining rate of approach to the asymptote.

tude of response (R_a), and speed of running or locomoting forward (R_s), are represented in the lower right-hand corner of the graph. Inside of the rectangle are represented the several intervening variables and the interrelations assumed among them.

We shall begin with the hypothetical learning factor, *H*. It will be assumed that underlying the response changes in each of these simple learning situations is a common factor, which will be referred to as habit strength (*H*). While acknowledging that this habit factor undoubtedly has some kind of physiological basis in the nervous system, we shall make no attempt to speculate as to its specific locus or detailed neurophysiological nature. Instead we shall limit our conception to the notion of a quantitatively changing capacity or property of the organism which, along with other factors, determines the strength of a response in any situation. More specifically, each time in one of these simple learning situations that a particular response occurs an increase will be assumed to occur in the capacity or habit strength of the contiguous stimulus to elicit the response.

It should be noted that this statement does not include as a condition for the development of habit strength the occurrence of a reinforcing event. While, as we have seen, the empirical law of effect implies that the strength of the tendency of a stimulus complex to evoke a particular response is dependent upon the occurrence or nonoccurrence of a reinforcer, it is not necessary to assume that the reinforcer affects the strength of this tendency through the hypothetical factor, *H*. The effects of the presence or absence of a reinforcer and of other kinds of variation of it, such as delaying its occurrence or varying its magnitude, may be conceived to affect response strength through other intervening variables than *H*. Actually, for our present purpose, it is not necessary to decide the question as to whether an increment in habit strength is contingent in any manner on the action of a reinforcer or whether it results merely as a consequence of the occurrence of a response. Our primary interest in the present chapter, it will be recalled, is the formulation of a quantitative theory that will permit the derivation of acquisition curves under conditions of consistent and adequate reinforcement.

Some comment is needed concerning the temporal relation of the stimulus event to the moment of occurrence of the response. Since the action of a stimulus on a receptor appears to involve a finite, perseverative aftereffect following its cessation, a stimulus event which is not contiguous with a response may

nevertheless acquire habit strength for it. It is only necessary that the perseverative sensory trace last until the response occurs. Presumably this experimental variable plays an especially important role in classical conditioning where it has been shown, at least in the case of such brief phasic responses as eyelid closure, that little or no conditioning will occur if the duration of the conditioned stimulus prior to the response is more than two seconds. Possibly the explanation of this finding is that the subject's receptor orientation is so controlled, i.e., so constant, in this type of situation that the afferent processes are not changing, i.e., are not dynamic after such a period. Obviously this is not the case in instrumental conditioning, in which the subject is moving in space or moving its head and hence producing more or less continuous changes in the effective stimulus.

Turning now to the mathematical specification of habit strength, we shall follow Hull (1943) in assuming that H increases as a negatively accelerated, exponential function of N. The equation, as shown in the box in the lower left portion of Figure 23, is $H = A(1 - 10^{-iN})$, where A equals the limit of growth of H and i is a constant determining the rate of approach of H to its asymptote. A and i are presumably parameters characteristic of individual learners.

Coming next to the intervening variable, D, representing the notion of the general drive level of a subject, we shall for the time being merely define D as some increasing monotonic function of the antecedent conditions determining the appetitional and aversive needs of the subject. In a later chapter we shall have occasion to elaborate this construct further in terms of the experimental studies that have involved the manipulation and combination of these motivational variables. On the basis of the experimental evidence considered earlier, we shall assume that D and H combine in a multiplicative manner to determine E, the excitatory potential or strength of the stimulus complex to elicit the response.

The next intervening variable, designated as I_o in the diagram, refers to the construct that Hull termed oscillatory inhibition and which he designated by the capital letter O. The basis for postulating this concept is the well-known observa-

tion that the behavior of organisms is highly variable. Even thoroughly learned, long established responses exhibit a characteristic oscillation from trial to trial in spite of the precautions of the experimenter to control all of the known determining variables. In his original treatment of this construct in the *Principles* Hull cited some evidence which suggested that the behavioral oscillations of an individual subject are distributed according to a normal frequency function, and on the basis of it he introduced the concept of an oscillating inhibitory factor, O, whose values were likewise assumed to be distributed according to this function.

While Hull offered certain physiological speculations as to the factors underlying these variations in response in terms of the variability of the action of neural fibers, they may simply be interpreted as reflecting the effects of a large number of uncontrolled factors in the system under observation. Thus in spite of the constancy of the physical environment the effective stimulus complex will vary with the receptor orientation of the subject. Likewise the motivational state of the subject will have slightly different strengths despite the consistency of the maintenance schedule and so on. Mathematically, the magnitudes of the effects of the joint action of such a multitude of independently varying factors would be expected to be distributed according to the normal probability function. If we now assume that these oscillating values affect the excitatory potential (E), we should also expect the latter to exhibit a normal frequency distribution and so likewise should any distribution of response measures linearly related to such an oscillating excitatory potential. Hull introduced the construct of *momentary* excitatory potential to represent the effects of this oscillatory inhibition on E and defined it as $\dot{E} = E - O$ or, in terms of the present terminology, $\dot{E} = E - I_o$. Since the original formulation in the *Principles* this concept of oscillatory inhibition has undergone a number of changes. Before considering these, however, I should like to complete a discussion of the remaining intervening variables in Figure 23.

Again in line with Hull's original formulation in the *Principles* we shall assume that a threshold value (L) of excitatory potential must obtain before a response will occur. Presumably

the value of L will vary with the response system involved and from subject to subject. Possibly it is also a function of such experimental variables as the length of a gap to be jumped or the intensity of a grid to be crossed. For the present we shall merely treat it as a constant for a given group of homogeneous subjects under a given set of experimental conditions (X). Subtraction of L from excitatory potential (E) or from momentary excitatory potential ($\dot{E}$) defines the concepts of a superthreshold excitatory potential ($E_L = E - L$) and momentary superthreshold excitatory potential $\dot{E}_L = \dot{E} - L$. The theory is finally completed by relating the several response measures to the intervening variable, momentary superthreshold excitatory potential ($\dot{E}_L$).

Returning now to Hull's more recent formulation (1952) of oscillatory inhibition we find a number of modifications. From the results of an experimental study (Yamaguchi et al., 1948) in which he obtained a distribution of 5,000 oscillating excitatory potential difference values as calculated by one of the psychometric scaling techniques of Thurstone, Hull came to the conclusion that the distribution of the oscillating E values was leptokurtic in form rather than normal. Based as it was on the pooling of data from 59 different subjects and involving a number of questionable assumptions in its determination, this conclusion has never seemed to me to be justified, at least as far as the variability of individual behavior is concerned. Obviously these data involved two sources of variation, the oscillation of behavior within each organism and individual differences among the organisms.

In an effort to obtain more adequate information concerning the nature of the behavioral oscillation function in individual organisms a doctoral investigation was conducted in the Iowa laboratory by Burke (1948). In this experiment Burke measured the time it took rats to traverse an eight-foot section in a straight runway over a period of approximately 100 trials after they had reached their performance asymptotes. Setting up certain criteria which excluded the data of subjects whose records showed any systematic trends or differences in behavior during this period, Burke found that seven out of a total of 27 subjects run met these criteria. Figure 24 presents

the cumulative variability functions or ogive curves for four of these seven subjects. The data represent the variability of speed-of-running measures as calculated from Burke's original running-time measures. The empirical values for each subject, shown by the solid dots, have been fitted by the integral of the normal probability function. It is readily apparent that the data of all four subjects show excellent fits as did the three subjects whose data are not presented. Indeed the subjects in the lower half of the figure represent the two poorest fits while those in the upper half represent the two best fits.

In the light of these findings we shall return to a conception of oscillatory inhibition more nearly like that originally put forward in the *Principles of Behavior*. Apart from Hull's physiological speculation, furthermore, oscillatory inhibition will be conceived in purely quantitative terms as varying in amount from instant to instant or from one time (T_R) to another according to a normal probability distribution, the variance of which, at least in the case of classical conditioning, is assumed to be constant for a particular subject under a particular set of experimental conditions. This variance will be different for different subjects and will also be a function of the degree of control of the experimental conditions, particularly those determining reception of the stimulus and the motivation of the subject. The term "instant," taken from Hull, needs clarification, and a comment is also required concerning the assumption that the dispersion of I_o is invariable throughout the course of a learning experiment, for Hull also abandoned this assumption in his later formulations.

With regard to the term instant, I have never been quite sure as to just what Hull meant by it. What, for example, is the duration of an instant? As I look back on my own understanding of the term I am now inclined to believe that my original interpretation was somewhat different from that of Hull. At least there appear to be two quite different notions. According to one, and I suspect that this is what Hull had in mind, the term refers to a duration at least as long as a trial period in classical conditioning. That is, the value of I_o during a given trial is assumed to remain constant with the value fluctuating from one trial to the next. A somewhat different conception is
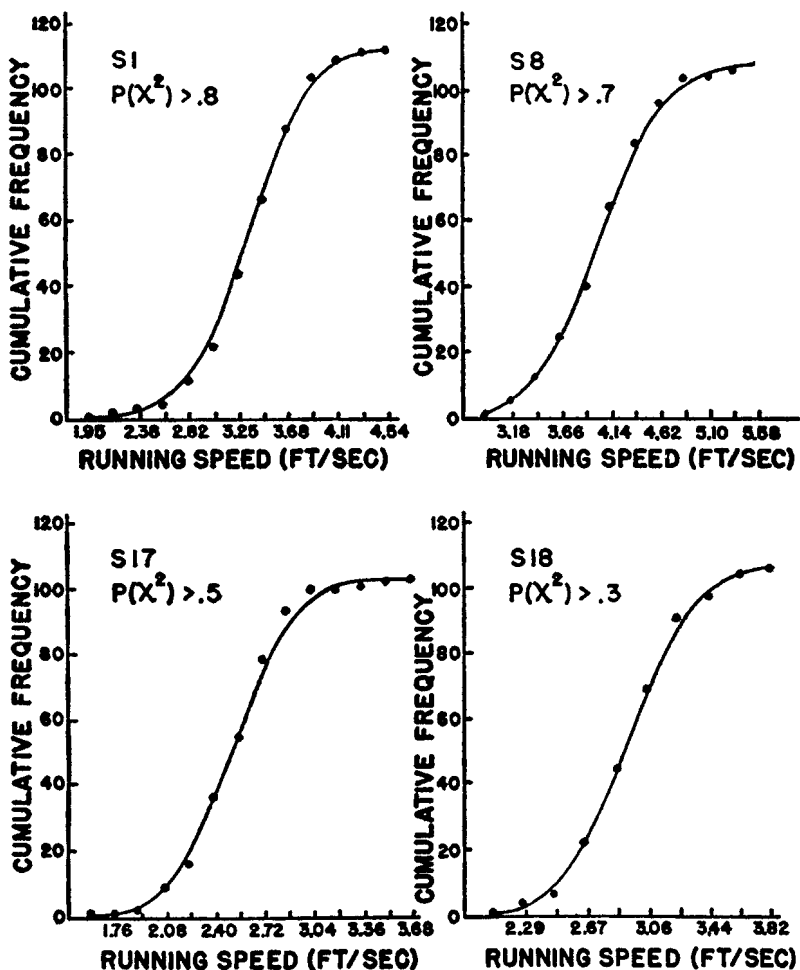
Fig. 24. Individual cumulative frequency distributions of asymptotic running speeds for four rats. Normal ogives are fitted to these data to test the deduction from oscillatory inhibition theory that performance at the end of acquisition will fluctuate from trial to trial according to the normal probability function. The results of chi-square goodness-of-fit tests are indicated in the upper left-hand corner of each graph. (Based on the data of Burke, 1948.)

that an instant refers to a very much briefer period of time, possibly a matter of a few milleseconds. According to this second conception the value of I_o would be changing during the period of exposure of the conditioned stimulus, oscillating from one value to another in accordance with the normal probability distribution. It was this latter conception that I employed in a recent article (Spence, 1954) to make certain derivations concerning the form of speed-of-response evocation curves in instrumental conditioning.

While this second conception has a number of promising features and I have found it useful in connection with certain phenomena of discrimination learning, nevertheless it presents a number of difficulties that I have never been able to resolve to my satisfaction. For the time being, then, I have shifted back to what I have interpreted to be Hull's original conception, namely that the value of I_o is, in the case of classical conditioning at least, more or less constant for the period or duration of each conditioning trial. The brevity of the interval of exposure of the conditioned stimulus in classical conditioning and the control that is maintained over the receptor-exposure adjustment of the subject make it plausible to conceive that the effective stimulus and other factors that determine variations in I_o are relatively constant during a single brief trial period.

The situation in instrumental conditioning, however, is much more complex. As was pointed out in Chapter 2 most instrumental conditioning situations are limiting cases of selective learning in which the occurrence of competing responses is at best minimized. Particularly in the early stages of this type of learning a number of different receptor-exposure acts leading to different overt acts may occur before the correct response leading to the reinforcer takes place. If, as we have assumed, lack of control of the receptor-exposure acts, and hence variation in the effective stimulus, is one of the factors responsible for behavioral oscillation, then it would follow that I_o would vary during a trial with the changes in the receptor-exposure acts. According to this conception not only would I_o vary within a trial but the range of magnitudes of I_o should be much greater during the very early trials of such learning. In later stages the correct receptor-exposure act becomes dominant and is the

first and only one to occur. Once this stage is attained the variations in the effective stimulus both within a trial and from trial to trial would be relatively small as compared to that in the early portion of such learning.

One implication of this reasoning is that whereas the *range* or *dispersion* of the values of I_o would be more or less constant during classical conditioning it would be relatively large at the beginning of instrumental conditioning and then decrease to a smaller, more or less constant value as the trial-and-terror receptor-adjustment acts are eliminated. These assumptions about I_o represent a marked departure from the assumptions made concerning the dispersion of oscillatory inhibition by Hull in his last formulations (Hull, 1951, 1952). Thus in his book *A Behavior System* Hull assumed that the dispersion of I_o begins with a zero value at zero value of habit strength and then increases to a maximum as learning progresses. Just what implications this latter assumption would have for instrumental learning is not too clear for, as discussed in the second chapter, the habit strength of the response learned in instrumental conditioning is probably well above zero at the beginning of learning.

THEORETICAL MODEL: RELATION OF RESPONSE MEASURES TO INTERVENING VARIABLES

Now that the task of defining these intervening variables has been completed, the remaining step in the construction of our theory consists in relating these hypothetical variables to the empirical measures of response strength.

Classical Conditioning: Frequency Curves. We shall begin with the curves of classical conditioning, and the first measure we shall treat is frequency or percentage of conditioned responses (R_p). This part of the theory is essentially identical with Hull's original formulation in the *Principles of Behavior*. It should be noted, however, that it is not necessary to introduce any additional assumptions in deriving the relation between this measure and reaction potential (E), for a necessary relation follows from the existing definitions of oscillatory inhibition (I_o) and the threshold, L. Thus it may be shown that the prob-

ability that the momentary reaction potential is superthreshold
is a normal integral function of the amount that E exceeds the
threshold, L (Spence, 1954). But this probability value, which
may be designated as $p(\dot{E}_L > O)$, also gives the probability of
the occurrence of a response, for L is defined as the value above
which a momentary excitatory potential will evoke a response.
That is, R_p will have the same value as $p(\dot{E}_L > O)$.
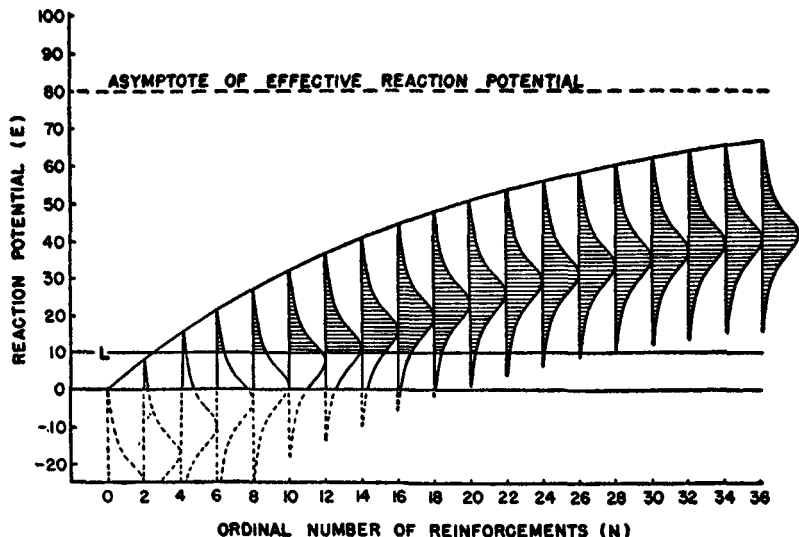
Fig. 25. Diagram showing the gradual movement of the oscillatory inhibition
distributions (upended bell-shaped areas) across the reaction threshold (L)
with the growth of reaction potential (upper curve). For further explanation
see text. (After Hull, 1943.)

If now the relation of E to N were assumed to be linear we
should also expect R_p to be a normal integral function of N. In
view of the fact, however, that E is assumed to be a negatively
accelerated, increasing function of N, it follows that R_p will be
a distorted *ogival* function of N with the initial positively ac-
celerated phase being shorter than the final negatively accel-
erated phase.

Figure 25, taken from Hull's *Principles of Behavior* (p.
327), shows how reaction potential, E, is conceived to develop as
a function of the number of conditioning trials. The series of

normal distributions represents the oscillatory inhibition. The shaded areas in each of these distributions represent the probability that the momentary excitatory potential ($\dot{E}$) will, on a particular trial, be greater than the threshold value, L. They also represent the theoretical probability of occurence of a conditioned response.
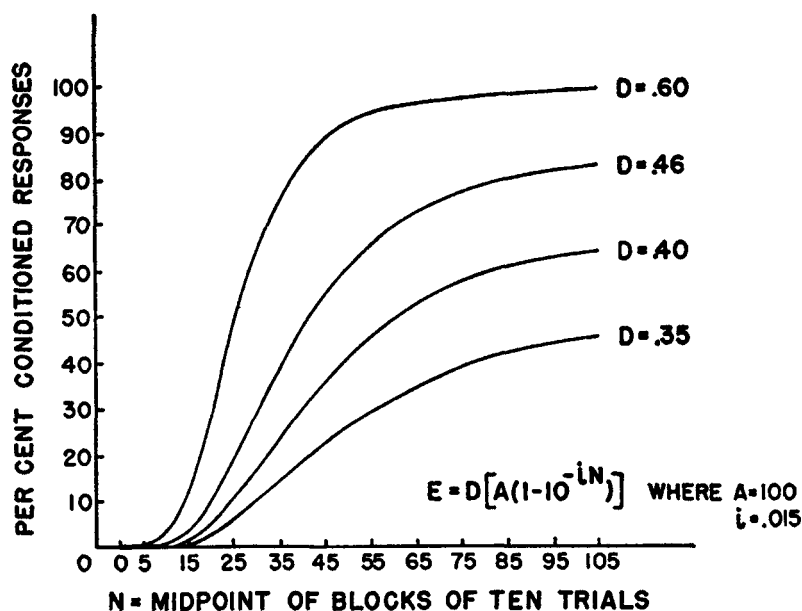
Fig. 26. A family of theoretical frequency curves of conditioning with drive level (D) as the parameter. The bracketed portion of the equation is $H = f(N)$ common to all of the curves. The theoretical per cent of conditioned responses (R_p) was obtained by calculating the values of E from the equation given above and then translating these E values to R_p by the use of oscillatory inhibition theory. For further explanation see text.

If we plot these theoretical response probability values (R_p) as a function of N for a number of different values of D in the equation describing the hypothetical growth of E, we obtain the family of theoretical frequency curves shown in Figure 26. As the parameters A and i in the equation for each of the curves have the same values, the different curves represent the theoretical effect of varying the level of D. Also since the initial

value of E was assumed to be zero and hence less than the threshold, each curve begins at a zero value of R_p and each exhibits an initial positively accelerated phase. Obviously negatively accelerated theoretical frequency curves could be obtained merely by assuming an initial E value of appropriate magnitude *greater* than the threshold value, L.

The general similarity of these theoretical frequency curves to the empirical frequency curves of eyelid conditioning reported earlier is readily apparent. The following series of graphs (Figures 27–29) provides more detailed evidence as to the extent to which the theoretical model fits the experimental data. While it would have been possible to have fitted theoretical functions directly to the empirical frequency curves, it is somewhat simpler, in that fewer parameters have to be estimated, if we transpose the empirical frequency measures into superthreshold reaction potential values. We can then ascertain whether or not the relation of these values to N conforms to the exponential function as the theory assumes. The translation of response frequency values into superthreshold values of E makes use of the theory of oscillatory inhibition. It is accomplished by means of a table of values of the normal integral function in which we consider the function as cut off at plus and minus 2.5σ. Thus if a subject gave six conditioned responses in a block of 10 trials, the value of the abscissa in the normal integral table corresponding to 60 per cent would provide a measure in units of the standard deviation of the oscillatory inhibition distribution of the average extent to which the reaction potential during the block of trials exceeds the threshold, L. Averaging these values for a group of homogeneous subjects gives a mean estimate of this value for each trial or block of trials. Shown in Figure 27 are two sets of such superthreshold reaction potential values $(E_L + 1/\sigma_{I_o})$ obtained for the middle 20 per cent of groups of subjects conditioned with a 5.0 lb. and .25 lb. per sq. in. air puff.[1] The solid and open circles are the empirical measures as transformed from the frequency values for 10 successive blocks of 10 trials each. Also given are similarly determined values

1. One σ_{I_o} unit was arbitrarily added to the superthreshold reaction potential values (i.e., $E_L + 1$) in order to eliminate negative values from the curves in the graphs. As a consequence L (threshold) has the value 1.

based on the first two trials. The solid curves are theoretical exponential functions that were fitted to the data by a trial-and-error procedure. However, instead of trying to obtain the best possible fit for each curve, an attempt was made to see how satis-
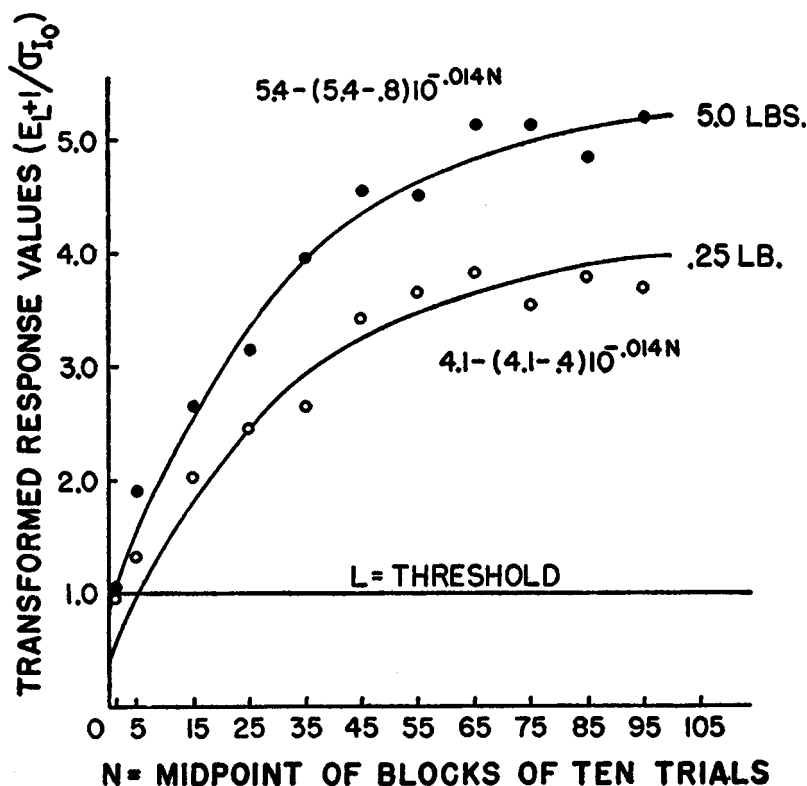
Fig. 27. Transformed frequency curves of eyelid conditioning for two groups of homogeneous subjects under different intensities of air puff. The circles are superthreshold reaction potential transformations of the original frequency data; the curves are exponential functions fitted with the restriction that the exponential constant be identical for both groups. One σ_{Io} unit was added to all scores to eliminate negative values, resulting in the arbitrary value, $L = 1$.

factorily both curves could be fitted when the same value for the parameter (i) determining the rate of approach of the function to the asymptote was employed. If, as Hull has suggested, this parameter reflects a purely individual difference

factor and if it is not a function of the different intensities of the unconditioned stimulus, we should expect its value to be the same for both curves. As may be seen, the theoretical curves provide a reasonably good fit to both sets of data.

Similarly, in Figure 28 exponential growth curves have been
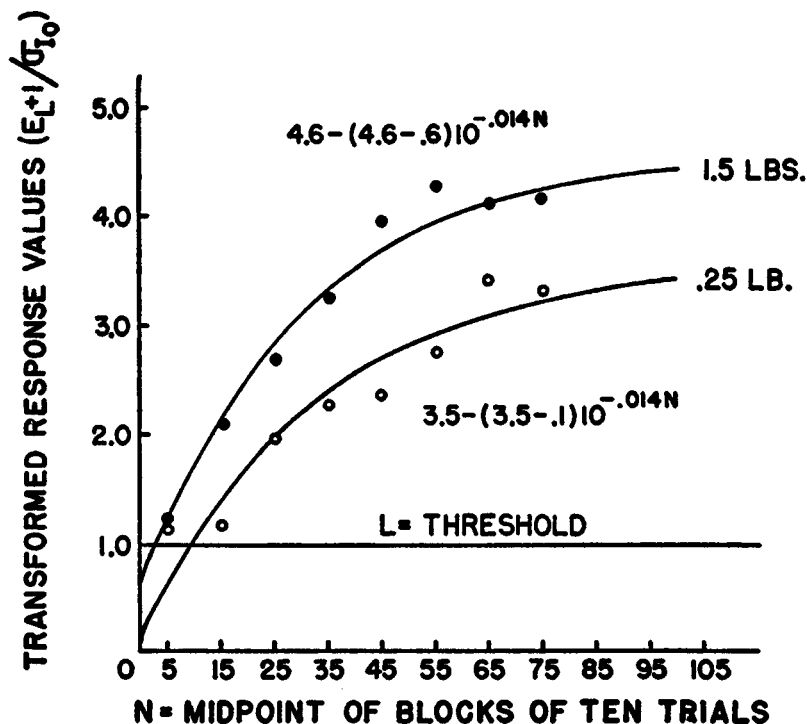
Fig. 28. Further examples of frequency curves of eyelid conditioning expressed in terms of the superthreshold reaction potential. The curves were fitted with the restriction that the exponential constant have the same value (.014) as that of the curves presented in Fig. 27.

fitted to the transformed reaction potential values of one of our most recent studies. In this experiment two groups of subjects were conditioned, one with an air-puff intensity of 1.5 lbs. and the other with an intensity of .25 lb. per sq. in. Unfortunately, only 80 conditioning trials were given, so it was somewhat difficult to estimate the asymptotic values of the data. It was at

least encouraging to find that theoretical curves employing the same value (.014) of the rate-of-growth parameter as in the previous graph fitted the data fairly well and required estimates of the asymptote that were in line with values from the other experiments.
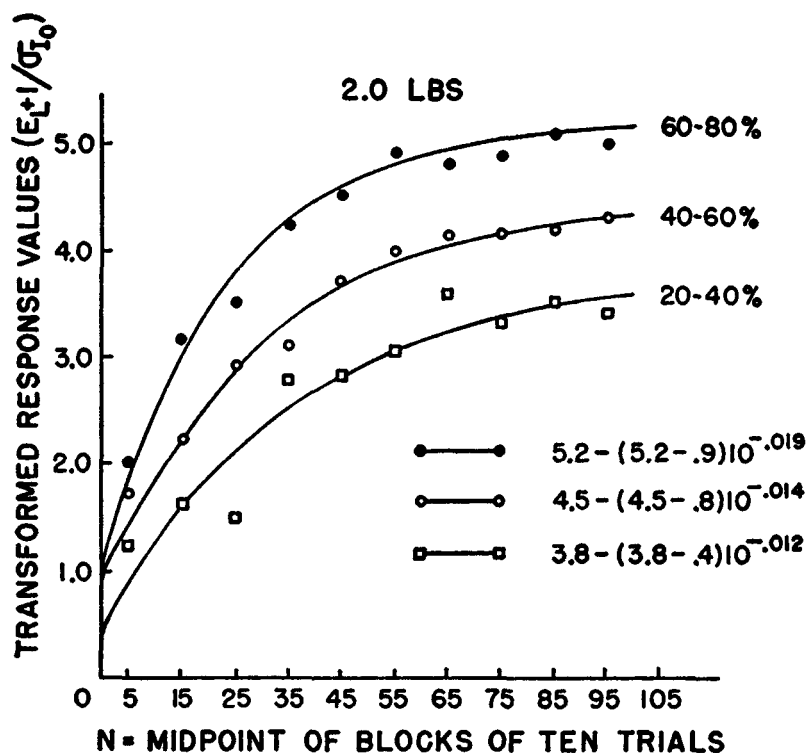
Fig. 29. Transformed frequency curves of eyelid conditioning for three homogeneous groups of poor (20–40%), average (40–60%), and good (60–80%) conditioners in terms of superthreshold reaction potential. Inspection of the equations fitted to these data reveals that the exponential constant, as well as the constant determining the performance maximum, is related to the total frequency of conditioned response occurrence.

The final graph of this series, shown in Figure 29, presents the data for three subgroups of "like" subjects, all of whom were conditioned with the same intensity of the unconditioned stimulus, an air puff of 2.0 lbs. per sq. in. The top curve is for

the subjects falling between the 60th and 80th percentiles of the total group, the middle curve for the subjects in the range between the 40th and 60th percentiles, and the lowest curve for the subjects falling between the 20th and 40th percentiles. Again the same value for the parameter determining the rate of growth of E (.014) has been employed for the middle 20 per cent group, and, as may be seen, a very good fit to the empirically determined points was obtained. It is of some interest to note that both the rate and asymptotic parameters vary for the subgroups from the different parts of the distribution. Thus the subjects showing the highest level of conditioning also have the highest rate of approach to the asymptote. Whether the higher level of performance reflects a higher value for the drive level parameter, D, or whether it implies a higher value of the parameter determining the maximum of habit strength (A) cannot be decided on the basis of these data. My own hunch is that it is primarily a function of the motivational level of the subject.

In concluding this treatment of frequency curves of classical conditioning I should like to call attention to one further implication of this theoretical model. A recent investigation reported by Voeks (1954) found that under certain experimental conditions some individuals, as a matter of fact half of her subjects, showed jumpwise frequency curves of eyelid conditioning in that their curves jumped from zero per cent response to one hundred per cent response in a single trial. That is, once the subject made an anticipatory response to the conditioned stimulus he continued on every subsequent trial to respond to it. That this phenomenon is an unusual one is attested by the fact that in the conditioning of 700 or so subjects in our laboratory in some dozen experiments, less than one per cent have responded on every trial subsequent to the first response even over as short a period as the ensuing 10 trials. In the few instances in which this has occurred it is perhaps interesting to note that the pattern of eyelid closure was of the form that we have identified as characteristic of the voluntary responder. In no case has a subject whose pattern of eyelid response was not of this voluntary form exhibited this perfect jumpwise curve.

Typical of our experimental data are the findings of our last

study which show that subjects averaged 7.3 response failures in the 10 trials immediately following the first conditioned response. It is obvious from this that our results are startlingly different from those of Voeks, and yet they are not necessarily contradictory for the experimental conditions were vastly different. Voeks specifically set out to control such variables as the effective stimulus, the postural adjustment, and the motivational set of her subjects to a far greater degree than in the typical conditioning study such as ours. It is precisely because all these factors have not been controlled that we have introduced the concept of oscillatory inhibition, I_o. The range and variance of I_o, it will be recalled, were assumed to be a function of the degree of failure to control all of the factors that can affect the response. That is, the more controlled the experimental conditions the less would the mean and variance of I_o be. Mention should also be made here of the fact that many of our experiments have employed a much weaker unconditioned stimulus than did Voeks since we have been interested in studying conditioning under low levels of motivation.

If we now examine the implications of our theoretical model for the probability of a conditioned response occurring in the trials subsequent to the first response it is readily apparent that the combination of conditions most conducive to a high probability (and hence, the possibility of a jumpwise individual curve) is when the magnitudes and variance of I_o are minimal and when E is rising sharply, i.e., when the increments of E per trial are maximally large. These conditions are produced according to our theoretical model when there is maximal control from trial to trial of the environmental conditions and the subject's set or postural adjustment and when the motivational conditions are such as to produce a high level of drive strength. These conditions, it should be noted, are precisely those that Voeks attempted to attain.

It is of further interest to note that a theory that assumed a gradually increasing value of the excitatory potential of the stimulus complex to evoke the response but did not include the concept of oscillatory inhibition would have no difficulty accounting for jumpwise curves of classical conditioning. In fact, such curves would always be predicted to occur. Such a theory

would, however, be singularly unsuccessful as far as the bulk of findings obtained in the usual conditioning experiments is concerned.

I should like to add here that the mathematical concept of a gradually increasing E that we have employed is not incompatible with the notion, such as that of Guthrie (1935), that the *elements* of the stimulus complex get hooked up in an all-or-none fashion with the response. Indeed, Estes (1950) has shown that a statistical theory based on this conception leads to the quantitative implication that the index of strength of the stimulus complex to evoke the response is an exponential function of the number of trials such as assumed here. My preference has been to begin at a more molar level, and I have not considered such matters as what underlies the increasing habit strength of the stimulus complex.

Classical Conditioning: Amplitude Curves. As we turn now to the implications of our theoretical model for amplitude curves of classical conditioning, it will be recalled that attention was directed in the last chapter to a number of factors that made for highly unstable conditioning curves employing this measure of response strength. In an attempt to provide some data based on "like" subjects, amplitude curves of eyelid conditioning were determined for subjects falling in the 60th to 90th percentile range of two groups. One of the groups was conditioned with a 1.5-lb. air puff, the other with a .25-lb. puff. This larger segment (30 per cent) of the total group was used in order to provide a larger sample of subjects, and the upper end of the distribution was employed because these subjects provide a larger number of conditioned responses, particularly in the early stages of training.

Figure 30 presents in the graph on the left the frequency curves of conditioning for the two groups of subjects in terms of the median per cent of conditioned responses occurring in successive blocks of 10 trials. Since the number of subjects involved in each case was 18 the curves are fairly smooth. Also, as the subjects were relatively rapid and high level conditioners, these frequency curves show relatively brief initial periods of positive acceleration just as our theory would demand. The graph to the right presents the amplitude curves for these same
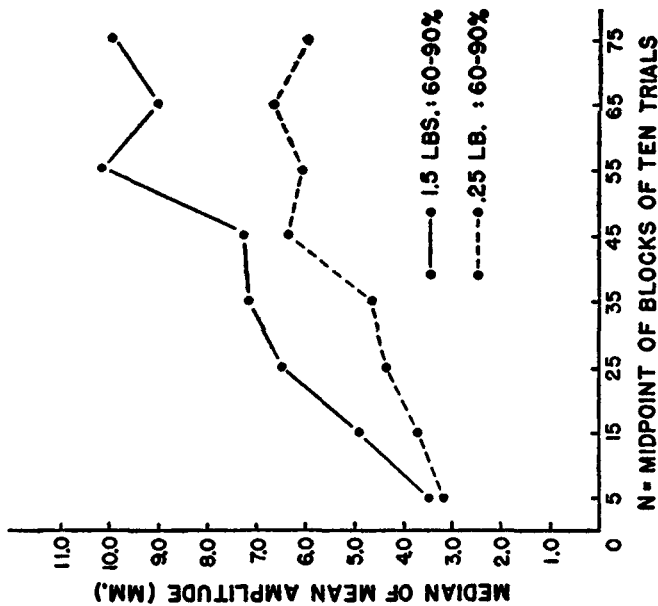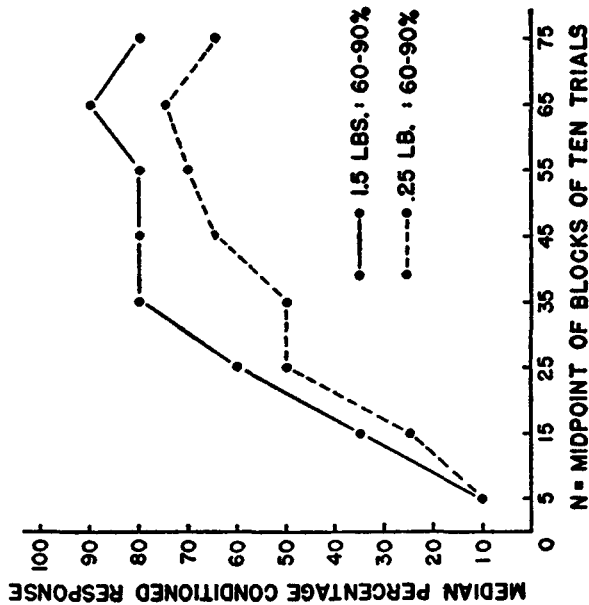
Fig. 30. Frequency (left) and amplitude (right) curves of classical eyelid conditioning for the same groups of homogeneous subjects, each trained under a different intensity of air puff. Subjects in both groups were selected from the 60th–90th percentile range in their respective distributions of total numbers of conditioned responses. Frequency values are the percentages of conditioned responses in successive blocks of 10 trials for the median subject. Amplitude values are the mean amplitudes of conditioned responses (zero values excluded) in successive blocks of 10 trials for the median subject.

subjects. Each point on this graph represents the median of
the mean of each individual's response amplitudes for a block
of 10 trials. Both curves, it will be noted, are surprisingly
regular, at least for this response measure. The lower one is
definitely suggestive of an ogive while the upper curve could be
fitted about equally satisfactorily by a negatively accelerated
or an S-shaped function.
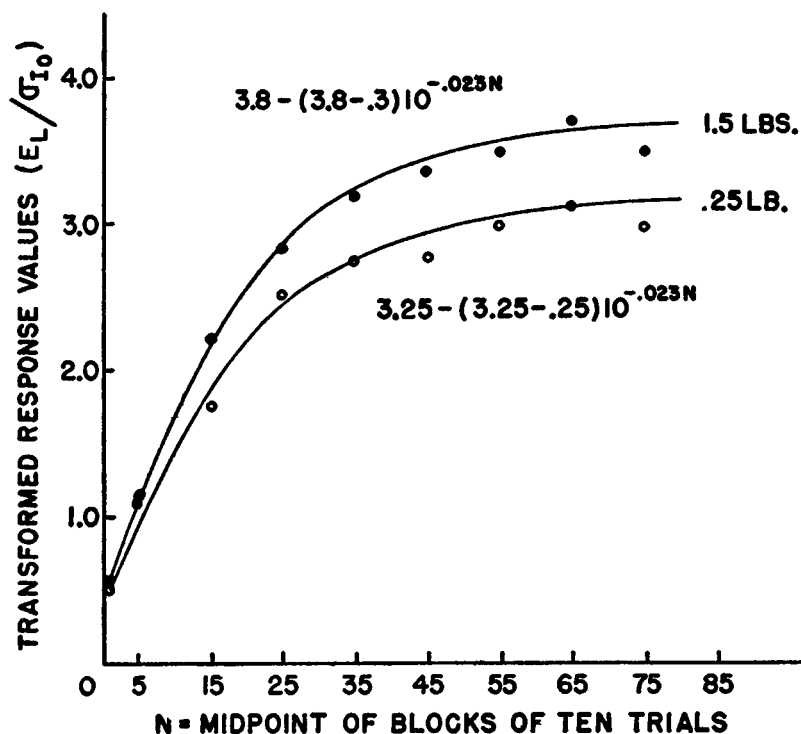
Fig. 31. The frequency curves of Fig. 30 transformed to superthreshold re-
action potential values. The smooth curves are exponential functions fitted with
the restriction that the exponential constants be identical.

Let us now consider the application of the theory to this type
of data. Shown in Figure 31 are the transformed or estimated
reaction potential measures and the theoretical exponential
curves fitted to them for these two groups. Again it will be noted
that the parameter, i, as in the case of the previous data, has

the same value for the two groups. Furthermore, it will be noticed that the value .023 is higher than any of the previous values, which is in line with the fact that the present samples include higher level conditioners than in the previous samples. The i value of .023, for example, is to be compared with the value .019 for the sample of subjects from the 60th to 80th percentile range.

Employing the fitted E curve for the 1.5-lb. group, Figure 32 attempts to show the manner in which we proceed to compute the mean of the distribution of superthreshold momentary E values. Introduced into the graph is the portion of the distribution of I_o values above the threshold, L, for every other block of 10 trials. The means of the truncated distributions above L represent the mean strengths of the superthreshold momentary reaction potentials, mean (E_L) in units of σ_{I_o}. These mean values may be calculated from the Kelley-Wood table of the normal probability integral by means of a formula provided by Kelley (1924). The broken line shown in the graph is drawn through these mean values as determined in this manner.

The implication of this theoretical curve for the response amplitude curve will depend, of course, on what function we assume holds between amplitude of response and reaction potential. If we assume that the relation is a linear one, then the curve drawn through the means of these superthreshold reaction potential values reflects the form of the theoretical amplitude curve as determined from the theoretical growth of the E curve fitted to the transformed or estimated E_L/σ_{I_o} values. The latter, of course, is based on the empirical frequency data obtained for the group of 18 subjects conditioned with the 1.5-lb. air puff.

As may be seen this mean superthreshold momentary reaction potential curve has the same general form as its corresponding amplitude curve shown in Figure 30. However, it is a simple matter to test this linear assumption between R_A and mean E_L/σ_{I_o}. Thus if we plot the empirical amplitude values obtained for each block of 10 trials against either the fitted or actual mean E/σ_{I_o} values for each block of trials we should obtain a linear function. Moreover, this curve must pass through the origin, for response amplitude must be zero for a zero value of

$\dot{E}_L/\sigma_{I_0}$. Figure 33 shows that this expectation was fulfilled, for as may be seen the plotted amplitude values do tend to fall along
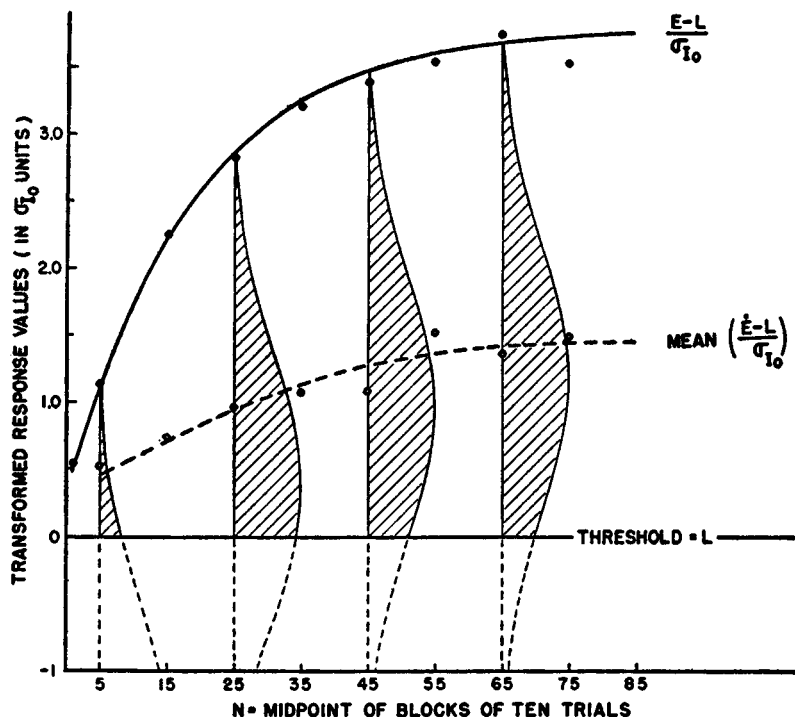
Fig. 32. Diagram depicting the interrelationships of response probability, response amplitude, and reaction potential as deduced from oscillatory inhibition theory. The upper curve is the superthreshold reaction potential function fitted to the transformed frequency data (filled circles) of the 1.5-lb. group shown in Fig. 31. The upended bell-shaped areas are the truncated oscillatory inhibition distributions, drawn five σ_{I_0} units in length as assumed by the theory. The dashed curve is drawn through the means of the shaded portions of these distributions, i.e., through the means of the superthreshold momentary reaction potential distributions. The hollow circles are mean superthreshold momentary reaction potential values (mean $\dot{E}$) derived from the amplitude data of the 1.5-lb. group (shown in Fig. 30) by means of the relationship between mean $\dot{E}$ and response amplitude depicted in Fig. 33. For further explanation see text.

a straight line passing through the origin in the case of both the .25-lb. and 1.5-lb. groups. Employing the slope constants (5.85 and 6.69) shown in this graph, theoretical amplitude
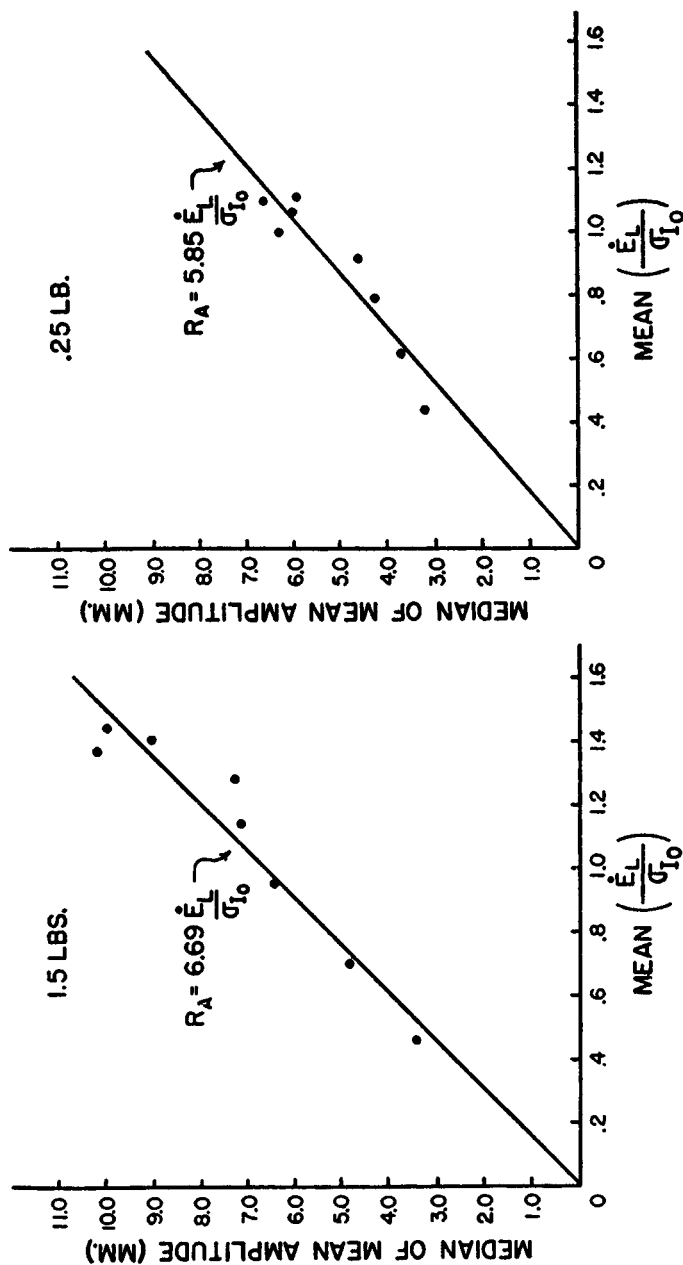
Fig. 33. Graphic tests of the assumption that response amplitude is linearly related to the mean of the superthreshold momentary reaction potentials as calculated on the basis of response-frequency data. Such plots of the data for the 1.5-lb. and .25-lb. groups shown in Fig. 30 are presented above. The two sets of points were fitted with linear functions by the method of least squares under the restriction that the lines pass through the origin.

curves determined as described from the frequency data were drawn through the amplitude data as a function of the conditioning trials. As may be seen from Figure 34 these theoretical curves have a short initial period of positive acceleration. While the experimental data are somewhat variable they conform quite well to the theoretical curves.
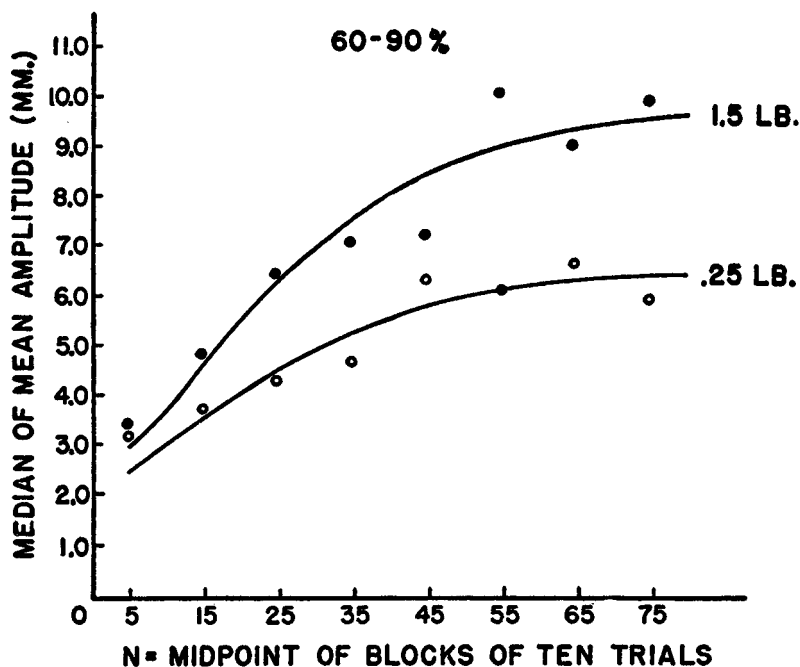
Fig. 34. Theoretical amplitude curves of eyelid conditioning. The circles are replots of the amplitude data shown in Fig. 30. The smooth curves were derived by use of the relationships depicted in Fig. 33 for mean superthreshold momentary reaction potential values calculated on the basis of the frequency data of these groups. For further explanation see text and Fig. 32.

Instrumental Conditioning: Speed-of-Evocation Curves. Turning now to the consideration of curves of instrumental conditioning, attention should be called again to the fact that in the early stages of such learning a number of different receptor-exposure acts leading to different effective stimuli and overt responses may occur on each trial before the appropriate or correct response takes place. Measures of response strength

involving the time required to *initiate* the to-be-learned response (response evocation latency and speed of response evocation) are thus a function not only of the excitatory potential of this response but also of the number and excitatory strengths of the competing responses. That is, these latter responses take time and hence increase the latency or decrease the speed-of-response-evocation measure when they occur. In the latter stages of learning when the correct response becomes sufficiently dominant in strength so that it always occurs first, these measures are a function only of the reaction potential of this response. If we now assume, as in the case of the amplitude measure, that speed of evocation of a response is linearly related to its excitatory potential, we should expect the later portion of curves involving this measure to correspond in form with the later stages of the growth of the mean superthreshold momentary E value, i.e., a negatively accelerated function.

Since the additional conditions, i.e., the absolute excitatory strength of the appropriate response and the number and relative strengths of the competing responses, are usually not known, it is not possible to specify in any particular instance the precise form of the early portion of such curves. If the initial excitatory strength of the appropriate response were low and if there were no competing responses we should expect such curves to have the same form as the theoretical amplitude curves shown in Figure 34. Actually the initial strength of the appropriate response is probably never low in such experiments, and there usually are, as was indicated above, strong competing responses present for some period of time. The effect of the latter would be to depress or distort downward the early portion of the curve, thus tending to produce functions with rather prolonged initial linear or even positively accelerated phases. As mentioned previously, curves involving the measurement of response evocation speed almost invariably are positively accelerated in their early portion. Instances of such curves are those presented in Figures 17 (p. 80) and 19 (p. 84).

Instrumental Conditioning: Speed-of-Response Curves. In this connection it should be noted that the speed-of-running measure is not so affected by these competing responses for in the case of this measure we do not start measuring until the

correct or appropriate response sequence has actually begun. Providing we confine our measurement to a fairly short distance and eliminate any trials on which competing responses are observed to interfere with the running response, the latter may reasonably be assumed to be a function only of its own momentary excitatory potential. Insofar as competition is successfully eliminated, then, speed-of-running curves should, according to our assumption that response speed is linearly related to reaction potential, reflect the growth of the mean value of the superthreshold E values much more precisely than curves involving the measurement of speed of response evocation. On the further assumption that the excitatory strength of such instrumental responses at the beginning of training is considerable, one might expect the initial positively accelerated portion of such curves to be absent. At least one can state that speed-of-running curves are much less likely to exhibit an initial positively accelerated phase than speed-of-evocation curves since the initial depressing effect of competing responses is removed from them.[2]

As far as the existing data, both published and unpublished, are concerned, speed-of-running curves have tended to be negatively accelerated in form, whereas speed curves involving evocation behavior have to a considerable extent tended to be either linear or positively accelerated in their early phase. The positively accelerated type of curve has been particularly prominent when the to-be-learned response has been relatively low in the initial response hierarchy. Unfortunately, however, the relevant evidence is as yet quite unsatisfactory. Experimental studies comparing curves of running speed and speed of evocation for homogeneous subjects under a variety of experimental conditions involving greater and lesser degrees of competition are needed before any theoretical model can be satisfactorily formulated and tested.

2. As pointed out elsewhere (Spence, 1954), if curves based on measures of evocation behavior tend to be only positively accelerated in their early stages while curves for speed of running are negatively accelerated, then curves based on a combination measurement of both evocation and running will tend to fall in between these two extremes. Attention should be directed here to the fact that a rather different theoretical model than that employed here was used in this earlier treatment.

A final point about these speed-of-response types of curves is that the deductions with regard to them assume that the growth of reaction potential (E) is an exponential one of the type Hull employed. This assumption in turn is based upon the postulate that H develops according to this function and that the other factors determining E, such as drive (D), work inhibition (I_R), and incentive motivation (K), remain constant during the training period. While there would appear to be experimental techniques that would guarantee a reasonable constancy of D and I_R, the incentive motivational factor (K), according to a theory to be elaborated in the next chapter (see also Spence, 1951a, 1951b, 1953b, and 1954), does not remain constant in instrumental reward conditioning but itself varies with the stage of training, i.e., the number of reinforced trials (N). That is, this motivational factor (K) is also regarded as being acquired or learned during the course of instrumental reward conditioning and hence as changing in value.

Considering now the implications of variation of K during learning, it may be shown that if K and H are each conceived of as starting at very low values and growing according to a negatively accelerated, exponential function then $E = (K \times H)$ would develop according to an S-shaped function with an initial phase of positive acceleration (Spence, 1954). Obviously, we need to take this function into account in making predictions as to the form of conditioning curves in instrumental reward conditioning.

An interesting implication of this part of our theory is that if one were to establish K at a maximum (hence constant) value prior to beginning the instrumental reward learning, the growth of E during the latter would be negatively accelerated throughout its course. According to the theory, as we shall see, K may be so established by setting up the classical conditioned fractional goal response that forms its basis prior to the instrumental training. Thus in the Skinner box the classical conditioned response $(S_c—r_g)$ of salivating, etc., to the sight and sound of the lever (S_c) would be established before learning the instrumental act of depressing the bar. Likewise, in the runway situation the subject could be given a large number of placements and feedings in the goal box previous to running in the

alley. If care is then taken to have the environmental cues of the runway highly similar to those in the goal box, the conditioned anticipatory feeding response (r_g) will generalize to the runway and thus be fairly strong before the instrumental learning starts.

Under this condition of a more or less constant and high value of K the growth of E would be negatively accelerated throughout, and hence speed-of-running curves would more likely be negatively accelerated from the beginning than if such prefeeding in the goal box were not given. While no direct comparison of speed-of-running curves under these two conditions has as yet been made, the curves of running speed shown in Figure 35 show that negatively accelerated exponential functions fit nicely the curves of two individual rats that were given such pretraining feeding in the goal box. In this experiment (Stein, 1955) running speed was measured for a distance of one foot in the middle of an alley which had a total length of four feet. Prior to the training the animals were given a total of 50 placements directly into the goal box in which they received one pellet of food. As may be seen the exponential function fits the data of both subjects quite well. Examination of the remaining subjects' data reveals similar-shaped curves although in most instances, particularly in female rats, the measures were much more variable than in these graphs.

Instrumental Conditioning: Resistance-to-Extinction Curves. The final type of conditioning curve we shall consider is that employing resistance to extinction as the measure of response strength. As yet this type of curve has been reported in only one kind of conditioning situation—instrumental reward. The evidence, such as that from the Perin-Williams studies, indicates that when response strength is measured in this manner it bears a negatively accelerated exponential relation to the number of reinforced trials in the original conditioning. Theoretically this would suggest that we assume that a hypothetical factor weakening reaction potential develops as a linear function of the successive nonreinforced trials (n). We shall designate this inhibitory factor as I_n and will assume that it acts in a negative, algebraic manner to weaken the excitatory potential of the response, the net strength after such weaken-
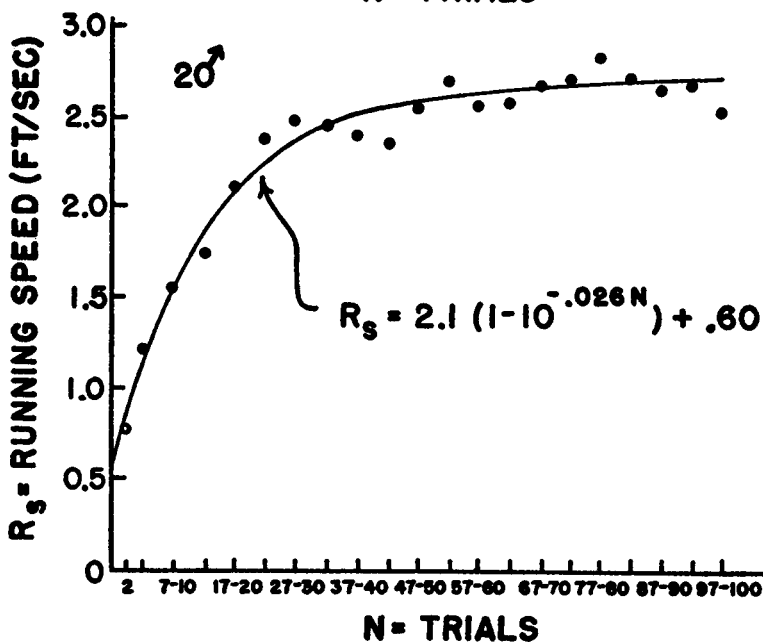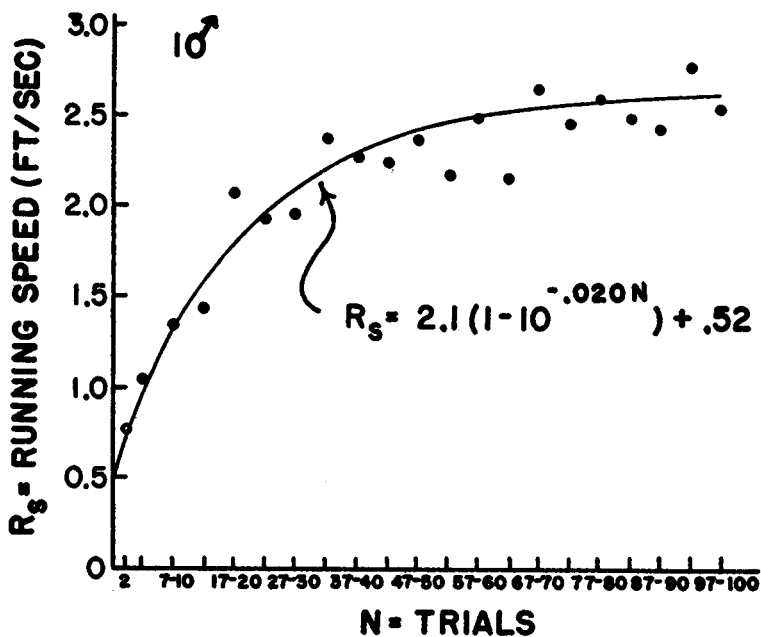
Fig. 35. Individual speed curves of conditioning of an instrumental running response for two male rats. The filled circles represent the mean running speeds for each daily session of five runs, first-trial speeds being excluded. The hollow circle in each graph represents the speed on the second training trial, i.e., the initial nonfirst-trial speed in the training series. (Stein, 1955.)

ing being designated as the effective excitatory potential, $E = E - I_n$ where $I_n = f(n)$. Attention should be called here to the fact that this concept I_n is not the same as Hull's work-inhibition notion, I_R. The operation specifying I_n is that of failing to provide for a reinforcer and not the amount of effort involved in making the response.

Unfortunately we are extremely short on information as far as the phenomena of response decrement with nonreinforcement are concerned, and much experimentation is needed before we can formulate a complete and satisfactory theory of inhibitory phenomena even in simple conditioning. I have the feeling that all the attempts so far made have been much too oversimplified in that other factors than some kind of inhibitory process affect response decrement. An obvious example of the latter is the reduction in the drive level (D) of the subject in the traditional extinction of a classical defense conditioned response with the removal of the noxious unconditioned stimulus. As McAllister (1953) has recently demonstrated, the extremely rapid decrement of the response under such circumstances is primarily a function of a decrease in the level of D and not due to the development of some kind of inhibitory factor. Similarly, in instrumental reward conditioning we shall have to consider the role of the incentive factor (K) in the weakening of such responses with the withdrawal of the reward. Instrumental and classical reward conditioning also involve frustration, which has long been a neglected factor in theorizing about response decrement with nonreinforcement in such situations. Finally, we are going to have to discover much more than we know now about the effects of interspersing reinforcements during extinction and vice versa in the discrete trial type of situation. While considerable experimentation has been conducted with partial or mixed schedules of reinforcement and nonreinforcement it has, unfortunately, mostly been carried on with the free-responding type of Skinner-box situation. It is my own belief that this latter phenomenon, important as it is, has little or nothing to do with extinction but rather is related to the problem of the length of the chain that can be established in instrumental sequences. Essentially what these studies have demonstrated is that one can establish very long instrumental chains.

Translated into the runway situation these types of studies are not nearly so dramatic as they appear in the context of the lever box, for what they have shown is that one can train a rat to take a great many steps, i.e., run a long way, to get a piece of food.

This completes our theoretical treatment of conditioning curves. As was mentioned several times the particular set of quantitative assumptions made was deliberately chosen so as to be able to derive the known empirical functions for the several measures. It is of some interest to note that exactly the same pattern or combination of assumptions was made in the case of all three response measures, speed and amplitude of response and resistance to extinction. That is, the relation assumed between reaction potential (E) and the particular response measure was in each instance a simple linear one. Similarly, a direct one-to-one relation was assumed between the frequency measure and the intervening variable to which it was related, namely the probability of a superthreshold momentary excitatory potential. Finally, in each case habit strength (H) was assumed to be an exponential function of the number of trials (N). The many different forms taken by conditioning curves under various conditions of experimentation and measurement follow from this theoretical schema and the particular initial or boundary conditions specific to each experiment.

5. The Role of Reinforcement in Instrumental Reward Conditioning

IN THE PRESENTATION of our theoretical model no consideration was given to the role of two experimental variables that specify certain parameters of the reinforcing event, namely its magnitude and the time of its delay. The assumption was made, it will be recalled, that the conditions of reinforcement were adequate and constant. In turning to the consideration of these variables it will be recognized that we are getting into an area in which there is a great deal of theoretical disagreement. On the one hand one group of theorists insist that learning requires the occurrence of some kind of reinforcing event, while opposed to this view is another group who deny that reinforcement is a necessary condition for the strengthening of a response. According to the latter group contiguity of the response with the stimulus event is a sufficient condition for such increments in response strength to occur.

Primarily concerned as I have been with the quantitative aspects of learning phenomena, particularly with the possibility of applying the quantitative laws of response change discovered in classical and instrumental conditioning to more complex learning situations, I have not been particularly interested in this issue. As I have pointed out on a number of occasions this type of quantitative theorizing can be carried on quite independently of such theoretical issues as reinforcement versus contiguity or even cognition versus associationism. Thus in a paper entitled "Mathematical Formulations of Learning Phenomena" I commented as follows:

> Considerable confusion has arisen from a failure to realize that these . . . quantitative theories of learning phenomena are, or can be, entirely independent of another

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class of learning theories, namely those concerned with the nature of the reinforcing process. Whereas the former theories attempt to provide guesses as to the laws governing the course of development of the hypothetical learning changes that occur with successive practice occasions, the latter are concerned with the conceptions as to how the unconditioned or reinforcing stimulus provides for the hypothetical change. The mathematical learning theorist can employ any one of these latter conceptions he wishes or he can completely ignore them. Thus he can be a reinforcement theorist of whatever variety (need reduction, drive-stimulus reduction, satisfier, etc.,) or a contiguity theorist of whatever type he desires (1952, p. 154).

But this concern with quantitative theorizing was only one of the reasons why for a number of years I have not been actively interested in the problem of how reinforcers act to increase response strength and why, unlike Hull, I was unwilling to go beyond the general or empirical reinforcement point of view. A second factor that played an important role in the adoption of this position was the series of experiments (Spence and Lippitt, 1940, 1946; Spence, Bergmann, and Lippitt, 1950; Kendler, 1947) on the simple T maze known as latent learning studies that was instituted at Iowa in the fall of 1939. While these experiments were primarily aimed at testing that part of Tolman's theory concerned with the development of sign-Gestalt-expectations or cognitions, it was hoped at the time they were started that they would also provide critical evidence on the question as to whether such learning could occur in the absence of a reinforcing state of affairs.

Almost completely lost sight of in the subsequent controversy over the findings of these studies is the fact that practically from the first we were forced to give up the second objective for the reason that we found it impossible to conduct an experiment without some form of motivation and reinforcing event. After a few exploratory runs the animals simply refused to perform. Subsequent to this we abandoned any further attempt to conduct the experiments without *some kind of reinforcing event* and turned to the investigaton of whether and under what con-

ditions animals could learn or acquire cognitions with respect to the locus of such goal objects as food and water on the basis of some other motivating state and reinforcing event, such as social goal objects, escape from confinement, escape from prodding, opportunity to explore, and so on.

Not only did these studies lead us to abandon any further attempts to find a crucial experiment on the reinforcement-contiguity issue, they also led to the formulation of an explanation of their findings in terms of the concept of the fractional anticipatory goal response, a notion that has played an important role in our thinking ever since. This formulation, which was first presented at meetings of the American Psychological Association (Spence and Lippitt, 1940) and the Midwestern Psychological Association (Spence, 1941), did not get into print until Kendler (1946) made a brief reference to it in explaining the results of one of the experiments conducted in our laboratory. A more extensive treatment was given in a later article (Spence, Bergmann, and Lippitt, 1950).

The essential notion of this formulation, as described briefly in Chapter 2, is that learning in instrumental reward sequences involves classical conditioning of the goal or consummatory response to stimulus cues throughout the chain as well as an increase in the habit strengths of the instrumental acts themselves. There is, of course, nothing new about this notion of the conditioned fractional anticipatory goal response. Indeed Hull originally introduced the concept in his early theoretical articles on maze learning (1930a, 1931), although he never described its possible role in simple instrumental conditioning. The major difference between Hull's use of this theoretical mechanism and mine is that I have regarded it primarily as a motivator and have identified it with Hull's more recently formulated incentive motivational construct, K (Spence, 1951a, 1951b). However, before considering the application of this theoretical mechanism to instrumental learning and its relation to magnitude of reinforcement, I should like to review briefly some of the findings with respect to the effect of variation of this latter variable and some of the interpretations of these findings.

PERFORMANCE AS A FUNCTION OF MAGNITUDE OF THE INCENTIVE (GOAL) OBJECT

Hull's Habit Differential Theory and Relevant Experimental Studies. The notion that an organism will perform at a higher level and learn faster for a large reward than it will for a relatively smaller one has been prevalent in the psychological literature for some time. In his *Principles of Behavior* Hull presented an analysis of the data of the few available studies involving variation of the magnitude of reward in simple conditioning and concluded that response strength did vary with magnitude of reward. On the assumption that a bigger reward object provided a larger amount of need reduction, Hull also put forward the hypothesis that the factor responsible for this increased strength of response with larger magnitudes of food was the learning factor, habit strength. Since the little available evidence indicated that the response measures went to different asymptotes with different reward magnitudes, Hull tentatively assumed that the limit (M) to which habit strength would develop varied as a negatively accelerated, exponential function of the magnitude of the reinforcing agent. The equation representing this assumption was as follows: $M = M_o(1 - 10^{-jW})$ where M = the limit of habit growth under the particular magnitude of reward object; M_o represents the asymptote of habit growth under optimal conditions of magnitude; W is the measurable property describing the quantity of the reinforcing object; and J is an empirical constant. When combined multiplicatively with the existing drive strength (D) this hypothesis led to the implication that curves of excitatory potential (E) and hence response measures linearly related to E would rise to different asymptotic values and that these latter would also be an exponential function of the quantity of the reinforcing agent.

Since these early investigations there have been a number of studies of the role of magnitude of reinforcement in instrumental conditioning. These studies have not only provided a more adequate picture of the influence of different reward magnitudes during learning but have also supplied us with information as to the effects upon performance level of shifts upward

and downward in the amount of incentive. As we shall see, these latter findings have had considerable theoretical significance.

Appearing just a few months prior to the publication of the *Principles of Behavior,* a study by Crespi (1942) furnished some timely data for testing Hull's hypothesis with regard to the action of this variable. Using an instrumental running situ-
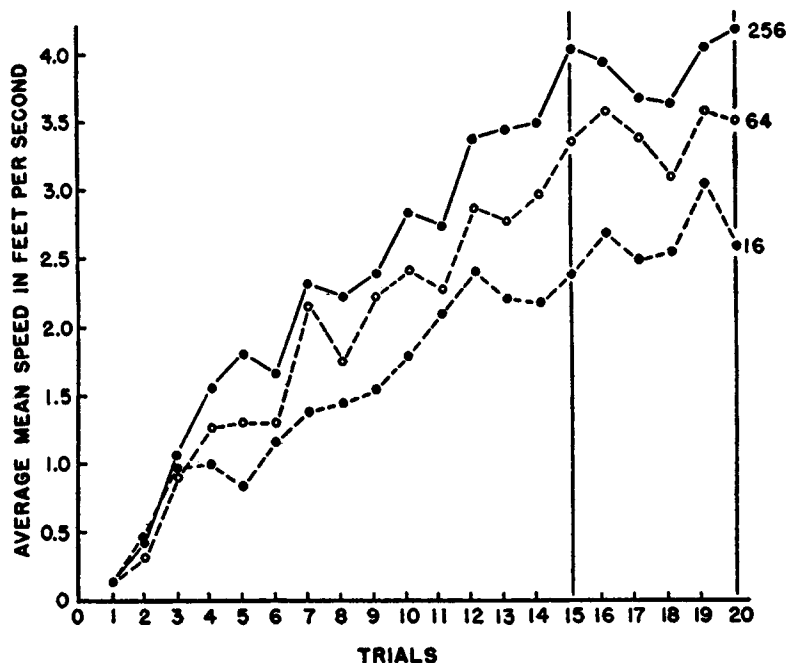
Fig. 36. Speed curves of running for three groups of rats with magnitude of food reward (gm./50) as the parameter. (Crespi, 1942.)

ation with white rats, Crespi measured performance in terms of running speed (including starting speed). In agreement with the mathematical implication of Hull's theorizing it was found that differential magnitudes of reward led to different levels of speed of running as is shown in Figure 36. Starting at the same level of performance, the curves gradually diverge with that for the largest reward being highest and that for the smallest reward the lowest. Accepting trials 15 to 20 as representing the asymptote of learning and combining the data of

two other experiments with those shown in this figure, Crespi (1944) found that the relationship between such "asymptotic" performances and amount of the incentive was a negatively accelerated, positive function except for a slight discrepancy at very minute amounts of reward. In a subsequent study in which an elevated runway was used Zeaman (1949) obtained further data supporting this type of relationship.
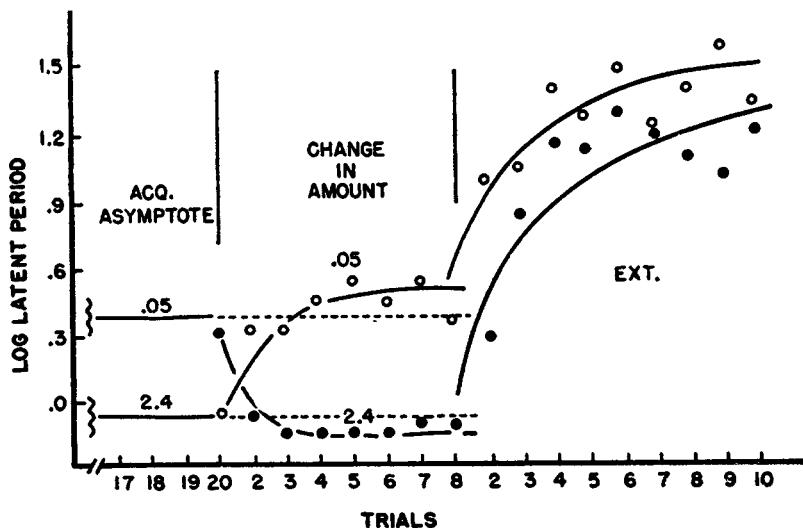
Fig. 37. The effect of reversing the amounts of incentive of two groups of rats trained to "asymptotic" latencies on .05 gm. and 2.4 gms. of cheese. The extrapolation of the prechange asymptotes as dotted lines provides a graphic picture of the extent of contrast effects, which take the form of discrepancies between the dotted lines and the solid curves drawn by inspection through the experimental points. (Zeaman, 1949.)

In addition to studying the effect of magnitude of the reinforcing object on level of performance both Crespi and Zeaman investigated the effects of introducing a *change* in reward magnitude. In general their findings indicated that an increase in reward size led to an increase in speed of performance while a decrease in quantity of incentive led to a decrease in performance level. Figure 37 presents the data obtained by Zeaman in terms of latency or starting times. If we confine our attention

for the moment to the data for the group in which incentive magnitude was increased, a number of aspects of the findings invite comment. First, it will be noticed that the shift in performance is quite sudden, the curve for the small-reward group shifting down to the level of that for the large-reward group after a single trial with the larger reward. Crespi's rats showed a similar rapid shift although they differed somewhat in that they continued to show an increase in performance speed throughout the nine trials of the postchange period. A second aspect of these data to observe is the fact that the curve for the shifted group went beyond the level that had been attained by the large-reward group. Crespi also obtained this same effect, and he gave it the name of "elation effect."

Turning to the effects of reducing reward size, Figure 37 reveals a parallel set of findings, i.e., a sudden increase in performance time to a level poorer than that of the group trained initially on the small reward. Crespi designated his similar finding the "depression effect."

With regard to the elation effect the question may be raised as to whether the higher performance level of the group shifted to the large reward might not have resulted from the fact that the asymptote of performance had not been attained at the time of shift and that the subjects were merely responding at a higher level because of the additional training. Studies conducted with the simple runway situation in which the training period has been more extended have shown that performance curves do not reach an asymptote until well beyond the number of trials given by Zeaman or Crespi. In the light of this fact a number of further studies were conducted in the Iowa laboratory with a view to ascertaining what the effects of such shifts would be after longer periods of training. In an unpublished study from our laboratory employing a runway 44 inches in length the results shown in Figure 38 were obtained.[1] The performance measure employed was the reciprocal of the time taken from the raising of a door in the starting box until the rat reached and pushed back a food panel exposing the food. One trial a day was given, and the subjects, hooded rats, were

run under 22 hours' food deprivation. One group (N = 10) received a 1.0-grm. pellet of food while the other (N = 11) obtained a pellet weighing only .05 grm. Training under these reward magnitudes was continued for 48 trials. On the 49th trial and for 10 subsequent trials the magnitudes of the rewards were reversed for each group. The points in the preshift train-
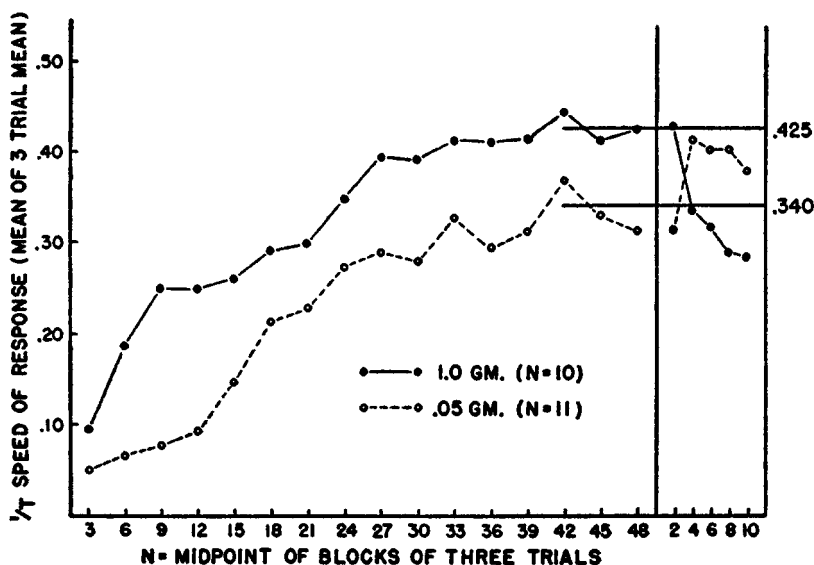
Fig. 38. Speed curves of a response chain involving locomotion plus panel pushing for two groups of rats trained on 1.0 gm. and .05 gm. of food reward, with reversal of incentive magnitudes following the 49th acquisition trial. The horizontal lines at .425/sec. and .340/sec. represent the estimated prechange asymptotes of the 1.0-gm. and .05-gm. groups respectively. A depression effect is evident, but no elation effect was obtained.

ing period represent the mean speed of response for successive blocks of three trials beginning with trial 2 and ending with trial 49. The points in the postshift period are averages of successive pairs of trials.

Looking first at the data during the training period it may be seen that curves for both the large- and small-reward groups continued to rise well beyond the point (19 or 20 trials) at which Crespi and Zeaman shifted their subjects. In agreement

with the finding of these earlier studies the performance of the large-reward group was consistently higher than that of the small-reward group, with the difference over the last nine trials being statistically significant. Examination of the curves following shift in reward magnitude also confirmed the sudden shifts in performance level. It will be noted, however, that the group shifted from the small to the large reward did not exhibit the elation effect. On the other hand, a depression effect similar to that obtained by Crespi and Zeaman was found.

The results of two further studies conducted in our laboratory, one a master's thesis (Czeh, 1954) and the other a second unpublished study,[2] have corroborated these postshift-period results. In neither instance was the elation phenomenon exhibited whereas the depression effect appeared in both experiments. The findings of these investigations in which the preshift training was more extensive than that of Crespi and Zeaman thus suggest that the elation effect resulted from the fact that the shifts occurred prior to the attainment of the asymptote of performance.

Returning now to the bearing of these experimental studies on the hypothesis originally proposed by Hull in his *Principles*, we have seen that the quantitative predictions concerning the performance curves during training were fairly well supported by them. However the implications of Hull's hypothesis with respect to the effects of change in the amount of incentive did not fare quite so well. With regard to the shift in the direction of a larger reinforcing object the hypothesis had a clear-cut implication which Hull himself treated at some length in the *Principles*. According to it an increase in the incentive amount would be expected to lead to an increase in the limit to which the habit strength would grow with the consequence that if the asymptote had been attained with the smaller reward there would be a rise in the performance curve which would continue until the new limit of habit growth was reached. While the results of all the studies are in agreement with this prediction as far as the direction of the change is concerned, the abruptness of the performance shift was not to be expected. Accord-

2. Unpublished study conducted with the assistance of Wallace McAllister and Donald Swisher.

ing to Hull's habit theory this change should have been a fairly gradual one resulting from the successive increments in habit strength. However, the shift to the new higher level occurred fairly abruptly in all studies, reaching or surpassing the higher level of performance characteristic of subjects that had been trained on the large reward in a very few trials.

The implication of Hull's theory with regard to reduction of incentive magnitude was never worked out in detail in the *Principles*. Hull merely stated that reduction of incentive size would be expected to lead on successive trials to a progressive lowering of performance level. Strictly speaking, Hull's hypothesis that the magnitude of H is a function of the magnitude of the reward, taken in conjunction with his assumption that habit (H) was a relatively permanent condition left by reinforcement within the nervous system, implied, as far as the value of H itself is concerned, that a shift to a smaller reward should not lead to a decrement in performance. However, the well-known fact that reduction to zero reward (experimental extinction) results in response decrement apparently led Hull to believe that any reduction in reward size less than to zero was related in some manner to experimental extinction and thus would result in some response decrement. Apparently he intended to return to this topic in the later chapter on experimental extinction, but he did not do so.

Incentive Motivational Interpretations of the Effects of Reward Magnitude. In subsequent formulations of his theoretical system Hull (1950, 1951, 1952) abandoned this hypothesis that habit strength varied with the magnitude of the reward and instead conceived of variations in this experimental variable as affecting the strength of an incentive motivational factor which he designated by the symbol K. Like the motivational factor, D, this K factor was assumed to multiply habit strength to determine the excitatory potential (E). In this modification of his theory Hull was anticipated by Crespi (1944), who on the basis of his experimental findings rejected Hull's differential habit interpretation and proposed in its stead an emotional drive or, as he anthropomorphically described it, an eagerness theory. According to this notion the basis of the differential drive strength was a variation in the

amount of anticipatory tension or excitement that developed with different amounts of reward.

My own approach to these experimental phenomena has also always been a motivational one. Indeed, at the time that Hull was writing the chapter on magnitude of reinforcement for his *Principles of Behavior* our correspondence reveals a vigorous disagreement over his learning (habit) interpretation. The basis of my disagreement, in part, was the finding of Nissen and Elder (1935) and Cowles and Nissen (1937) with respect to variation of the magnitude of the goal object in delayed-response experiments with chimpanzees. After showing that the level of performance in this situation was a function of the magnitude of the incentive, these investigators further demonstrated that, after attaining a certain level of response with a given size of food, a drop in level of performance occurred if a smaller piece of food was used. Similarly, a shift to a larger piece of food was shown to lead to improvement in performance. These shifts up and down seemed to me to suggest changes in a motivational rather than a habit factor, and it is interesting to note that Cowles and Nissen interpreted their findings in terms of a mechanism which they described as reward expectancy.

My preference for a motivational interpretation was also greatly influenced, as I have already indicated, by our theorizing concerning the role of the fractional anticipatory goal response in our latent learning experiments with the simple T maze (Spence, Bergmann, and Lippitt, 1950). This theory assumed, it will be recalled, that stimulus cues in the goal box and from the alley just preceding the goal box become conditioned to the goal response, R_g. Through generalization the stimulus cues at earlier points in the runway are also assumed to acquire the capacity to elicit R_g, or at least noncompetitional components of R_g that can occur without the actual presence of the food (e.g., salivating and chewing movements). As a result this fractional conditioned response, which we shall designate as r_g, moves forward to the beginning of the instrumental sequence. Furthermore, the interoceptive stimulus cue (s_g) produced by this response also becomes a part of the stimulus complex in the alley and thus should become conditioned to the

instrumental locomotor responses. But more important, in addition to this associative function we have assumed that this r_g-s_g mechanism also has motivational properties that vary with the magnitude or vigor with which it occurs.

A number of different conceptions of the manner in which the r_g-s_g mechanism may operate to affect motivational level have been suggested. One that was mentioned in my chapter on theories of learning in Stevens' *Handbook of Experimental Psychology* (Spence, 1951b) was that the occurrence of these fractional goal responses results in a certain amount of conflict and hence in heightened tension or excitement. This heightened tension, it was assumed, might contribute to an increase in the existing state of general drive level, *D*. This conception, it will be seen, is very similar to that of Crespi. Another possibility is that variation of the intensity of s_g provides an internal stimulus dynamism akin to Hull's notion of stimulus dynamism (*V*) resulting from different intensities of external stimulation. However, my preference is merely to introduce an intervening variable, *K*, which is regarded as representing, quantitatively, the motivational property of the conditioned r_g-s_g mechanism and which is defined in terms of the experimental variables that determine the vigor of the latter.

From our assumption that the basic mechanism underlying this incentive motivational factor, *K*, is the classical conditioned r_g, we are necessarily committed to a number of assumptions as to the variables that determine its strength. Thus, being itself a conditioned response, r_g will vary with the number of conditioning trials given in the goal box. Secondly we must assume that its intensity or vigor will be a negatively accelerated, exponential function of the number of these conditioning trials. Furthermore, on the basis of experimental studies of generalization of conditioning we will need to assume that its strength at any point in the alley distant from the goal box will be a function of the similarity of the environmental cues at that point and those in the goal box. If internal, proprioceptive cues from the running response play an important role, the differences in these cues at different distances from the goal box will have to be considered. Unfortunately we know very little, as yet, concerning either of these variables. Finally, any prop-

erty of the goal object that produces unconditioned consummatory responses of different intensity or vigor will presumably determine the value of K, for there is some evidence to support the notion that the intensity or vigor of the response may be conditioned *as such* (Hull, 1943).

The diagram shown in Figure 39 attempts to summarize these various assumptions relating K on the one hand to the experimental variables of which it is a function and on the other
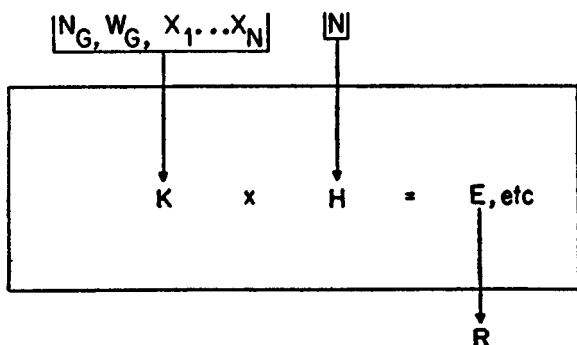
Fig. 39. Diagram summarizing some of the assumptions of incentive motivation theory. Some of the experimental variables which are assumed to contribute to the level of incentive motivation (K) are shown above the rectangle on the left. The multiplicative assumption of K and H combination is shown inside the rectangle. For definition of symbols and further explanation see text.

to the intervening variables and behavior. Actually, as will become apparent, considerable more research needs to be done before we can specify in a systematic and precise manner the experimental variables that affect K. The listing in the diagram is in terms of the specific experimental operations that different investigators have so far employed. Thus N_G refers to the number of classical conditioning trials, which has typically been designated as the number of times the subject enters the goal box and responds to (consumes or sees) the goal object. W_G refers to the amount, e.g., the number, weight, or volume, of the goal objects consumed, while X_1—X_n refers to a number of known and as yet unknown variables that presumably deter-

mine the vigor of the consummatory response, e.g., the sweetness of the object, the amount of sucking effort required to obtain the object in the case of liquids, possibly the hardness of the reward object, and so on. Again, as we shall see, these variables are not entirely independent of one another. The tentative hypothesis being proposed here is that these different experimental variations of reinforcing agents will determine K, either through the habit strength of r_g or through the particular r_g, i.e., the particular vigor of r_g being conditioned.

Having defined K in terms of these experimental variables, we must next relate it to our other intervening variables and thus eventually to the response variables. Being conceived of as a motivational factor, K is assumed to multiply the habit strength of the instrumental response to determine its excitatory strength. Thus, with D held constant E equals $K \times H$. This is not, of course, a complete picture of our current theorizing concerning K. Conspicuous by its absence is the role of the drive stimulus (S_D) from the particular need state related to the consummatory response. Nor is the manner in which K and D interact or combine indicated. For the present we shall consider only the experimental variables relating to the properties of the reinforcing objects and the manner in which they are varied.

Experimental Analysis of the Magnitude Variable. From the point of view of the hypothesis offered here a major problem is that of ascertaining the nature of the relations existing between the experimental manipulations of the reinforcing object and the two ways in which the vigor of the r_g might conceivably be varied. Do, for example, two different magnitudes of the goal object in a goal box lead to the same amount of conditioning (i.e., same habit strength) of two r_g's which differ in vigor? Or, does the fact that the subject spends more consummatory time in the goal box in the case of the larger reward mean that the difference in vigor results from a greater amount of conditioning of the r_g to the cues in the goal box than in the case of the small reward? These questions have to do with the nature of the classical conditioning of the consummatory response in the goal box with variations in the magnitude of the reward object, the number of different pieces of the object, the

time and vigor of the consummatory activity, and so on. Unfortunately, there is little or no direct evidence on these questions from classical reward conditioning studies themselves so we are entirely dependent on information from instrumental conditioning data.

As far as experimental variation of the amount of food is concerned, most investigators have used a single food particle differing in weight or size. The next most frequent variation has involved different numbers of food particles of equal weight or size. A few studies have varied the period of exposure to the food. All three types of variation have produced differences in the strength of the instrumental response. Because all previous investigations involving different amounts of the reward object have also involved different consummatory time in the goal box we recently conducted some studies in which the time variable was controlled. In a doctoral investigation employing a modified form of the Skinner box, Swisher (1951) gave one group of rats a single piece of food weighing 2.5 grms. and a second group a smaller piece of food weighing only .05 grm. Animals receiving the small reward were allowed 30 seconds in the apparatus to consume all of the reward pellet. Those receiving the large reward were also permitted to eat for only 30 seconds in the apparatus. They were then removed with what remained of the food pellet to the carrying case where they completed eating. Performance curves in terms of the reciprocals of the response latencies showed no difference over the 40 training trials, as Figure 40 shows. The second study, a master's thesis (Czeh, 1954), gave the same result for speed of response evocation.[3] In this experiment, the results of which are presented in Figure 41, three different groups were trained, two with a relatively large pellet of food (1.5 grms.) and one with a small pellet (.10 grm.). One of the large-pellet groups (I) was allowed to consume all of the pellet in the goal box, the time re-

3. Somewhat complicating the picture are the results for the speed-of-running measure reported by Czeh. In the case of this measure he found no differences between the three groups for the first 15 trials, after which the group (II) that had the large piece of food and ate for 30 seconds in the goal box ran significantly slower than the other two groups. The latter continued to respond at the same level of speed.

quired being about four minutes on the average. The second
large-pellet group (II) was given only 30 seconds in the goal
box, the same period being permitted the small-pellet group
(III). At the end of this period the subjects in group II were
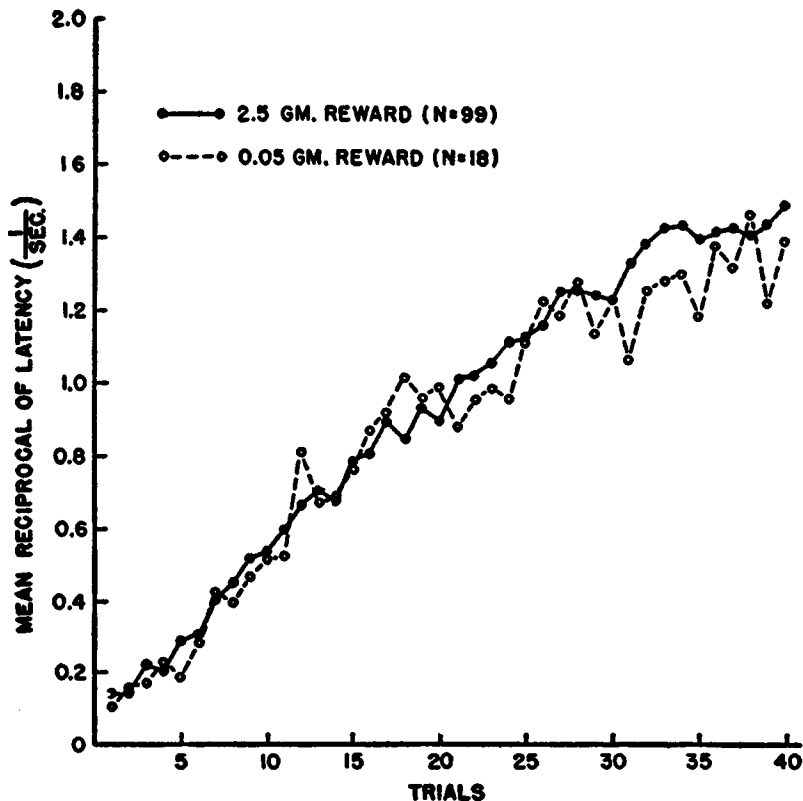
Fig. 40. Speed curves of bar-pressing for two groups of rats trained on
2.5 and .05 gm. of food reward. Eating time *in the goal box* was held constant
at 30 secs. for both groups. (Swisher, 1951.)

shifted from the goal box to a feeding cage where they finished
eating the food pellet.

The results of the learning period clearly reveal that speed
of starting in the runway varied with consummatory time in the
goal box and was not a function of the size of the pellet per se.
Thus there was no difference in the behavior of groups II and

III, each of which had 30 seconds' eating time in the goal box. Group I, which had the same size of pellet as group II but was given a longer period in the goal box, was significantly superior to either group II or group III.

One interpretation of these findings would be that the same magnitude of r_g was conditioned under the different reward
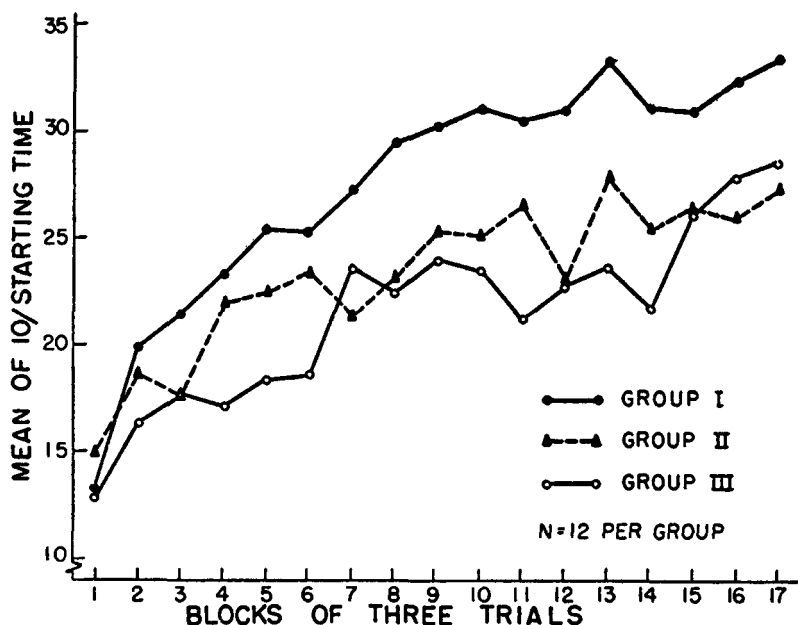
Fig. 41. The effect of incentive magnitude on instrumental performance relative to eating time in the goal box. The ordinate represents the mean speeds of evocation of a running response for groups of rats under three conditions of incentive magnitude and eating time. Group I was permitted to consume entirely a 2.4-gm. food reward, averaging 240 secs. in the goal box; group II also received a 2.4-gm. reward but was permitted only 30 secs. in the goal box, and group III received .1 gm. with 30 secs. of consummatory time. (Czeh, 1954.)

conditions but that more conditioning of it occurred, i.e., a greater habit strength was developed, in the case of the subjects that were in the goal box for a longer period. Fortunately, there is a testable implication of this particular hypothesis, namely, that if training is continued for a very long period the performance curve for the subjects receiving the smaller par-

ticle should eventually reach the same asymptote as that attained with a group given a larger reward and allowed a longer consummatory period. This follows from the assumption that the growth of habit strength of the two conditioned r_g's reaches the same limit or asymptote. At the present time there are no data relevant to this prediction since it would require training subjects for a much larger number of trials than any investigator has as yet done.

Another and possibly more sensitive test of this hypothesis would be to employ a simple T maze, in one arm of which the subject receives a large piece of food and has a relatively long period of eating time on each trial while getting a small piece of food and a relatively short period of time in the other. If free and forced trials were employed so as to keep the number of trials to each side equal, it may be predicted that the subject would at first develop a preference for the side containing the larger reward but that eventually this preference would disappear. For reasons that will become apparent later it is extremely important that sufficient food be available on the small-reward side so that the subject will not consume all of the food prior to the period of time allowed in the goal box.[4]

I should like to add here that I am not entirely convinced that a repetition of these studies of Swisher and Czeh with special care being taken to insure continuous eating by the subjects in the goal box will produce the same results. My reason for these doubts springs from an observation reported by Czeh to the effect that most of the subjects in the group given a large pellet of food but limited to 30 seconds in the goal box developed a tendency, after picking up the food, to wait to eat until they were placed in the transport cage. Unfortunately the conditions of observation were not such as would permit careful evaluation of this factor. Repetition of this type of experiment under conditions that will provide for

4. Evidence somewhat unfavorable to this hypothesis is provided by an experiment reported by Festinger (1943). This investigator found that rats chose the side on which they were fed 60 seconds over one that permitted 10 seconds feeding 100 per cent of the time after a total of 96 trials (24 days). However the equalization of trials was not complete as six of the 24 days involved free choice on all four trials, while 18 involved forcing to each side twice.

a careful check on the course of eating in the goal box is needed.

The same questions arise when we turn to the procedure in which variation of the amount of the reward object received is in terms of different numbers of particles of equal size. Is a more vigorous r_g being conditioned in the case of the larger number of pellets, or is there just more conditioning of the same magnitude of r_g per given trial because of the longer period of eating? In addition to the indirect types of experimental test suggested previously it would seem feasible to measure the vigor or voracity of the consummatory response of rats when confronted with different numbers or sizes of food pellets. In this connection it is interesting to note that the German psychologist David Katz (1937) reported an experiment in which it was shown that chickens eat twice as much when confronted with a very large heap of grain than with a smaller heap. Thus it was found that a hen placed before a heap of 100 grains of wheat ate on the average 50 grains. When placed before a larger heap under the same hunger condition it consumed from 85 to 104 grains. Unfortunately Katz did not report whether the rate of eating was faster or whether the subjects merely ate for a longer period of time.

In addition to varying the magnitude of the reinforcing object, two recent studies reported by Guttman have investigated the effect of using different concentrations of sucrose as the rewarding agent. In the first experiment Guttman (1953) found that the acquisition, resistance to extinction, and rate of responding in a lever-depressing situation under continuous and periodic reinforcement were positively related to the concentration of sucrose. Figure 42 shows the nature of the relation found to hold between the rate of responding and the logarithm of the sucrose concentration during tests involving periodic reinforcement every minute. It should be noted that the data represented by the solid curve involved testing the subjects at each of the concentration values. These data show that, just as in the case of the experiments in which the magnitude of the reward was changed, rate of responding shifted up and down with changes in concentration. It is interesting to note, moreover, that no evidence was found either for the elation effect or for the depression effect. Guttman explained the absence of the de-
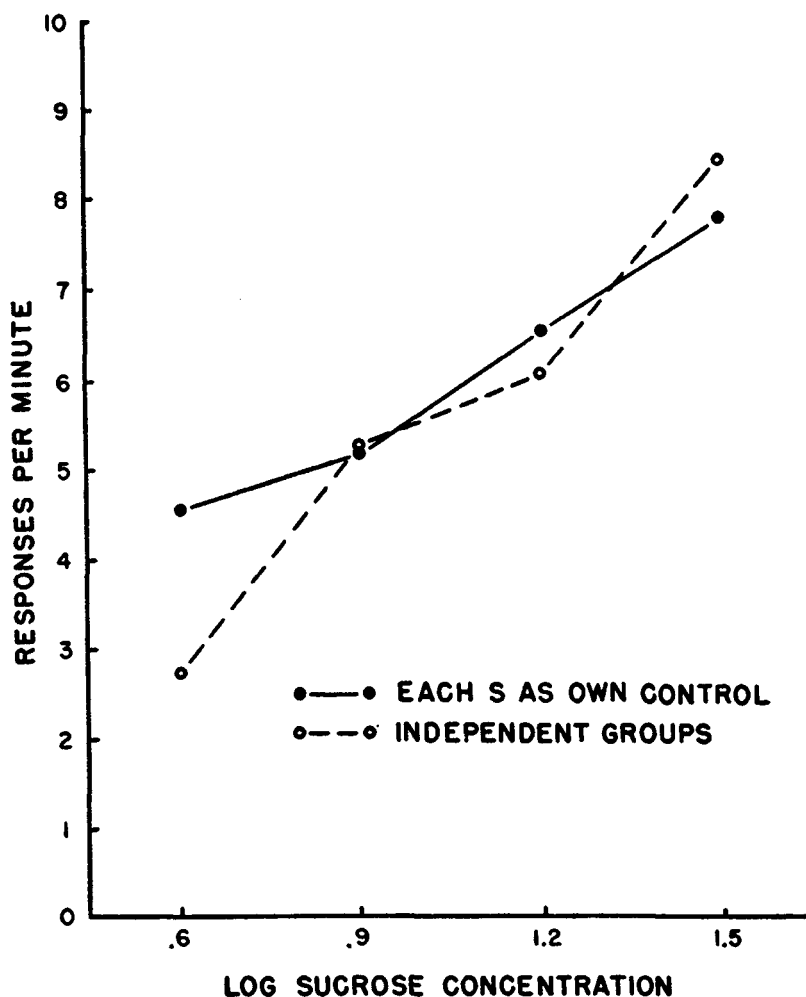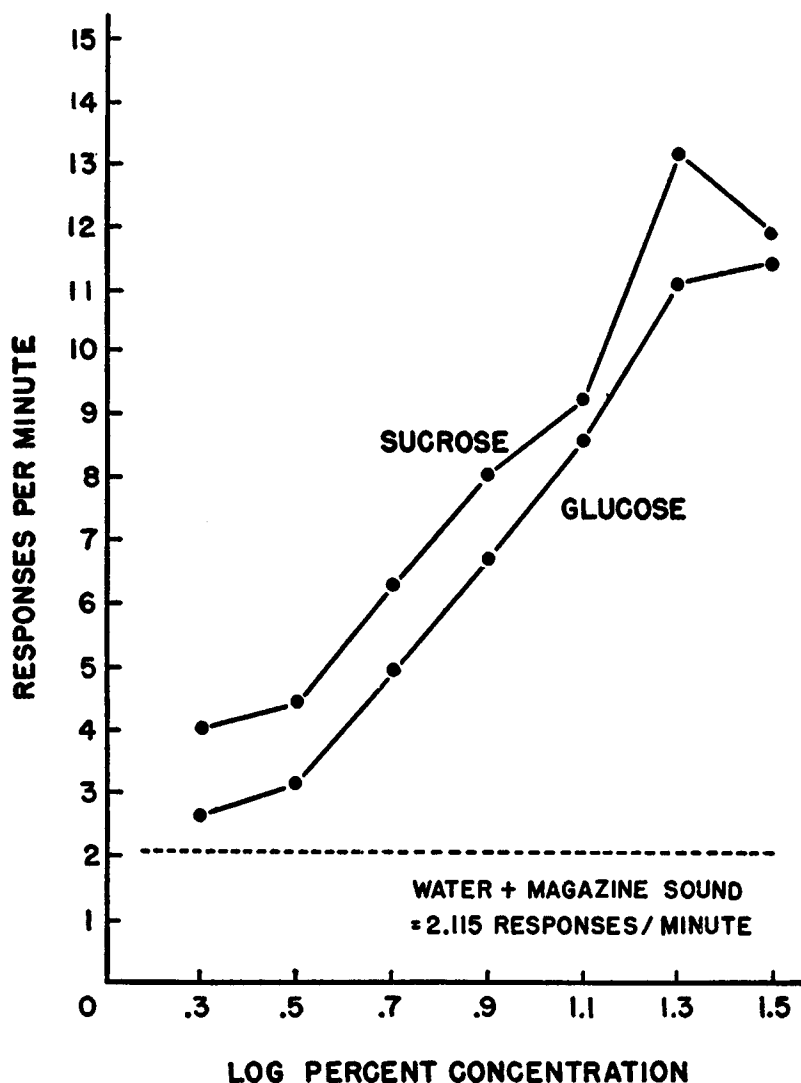
Fig. 42. Mean rate of bar pressing under periodic reinforcement (1-min. interreinforcement interval) as a function of the log concentration of a sucrose reward. The solid line represents the data of single groups of rats trained and tested under varying concentration; the dashed line represents the data of different subgroups, each trained and tested on one concentration. Differences between the two sets of data were found statistically to be nonsignificant. (Guttman, 1953.)

pression effect as possibly resulting from adaptation to the effects of decreasing the concentration over a number of such changes. This would be in line with the interpretation that Crespi (1944) offered to the effect that the depression effect is an emotional or frustration phenomenon and hence temporary in its nature.

In a second experiment Guttman (1954) compared speed of responding in the same type of instrumental conditioning situation as a function of the concentration of two different reinforcing agents, sucrose and glucose. Our particular interest in these data is related to the fact that for any given concentration value of these substances sucrose is judged by humans to be sweeter than glucose. Furthermore, it is known that rat preference thresholds and human sweet-taste thresholds for these substances are comparable. Figure 43 shows the relation holding between rate of lever depression under aperiodic reinforcement and type and concentration of the reinforcing agent. It is apparent that the sweeter substance, sucrose, produced a higher rate of responding.

The manner in which these experimental findings fit into our incentive motivation hypothesis is perhaps obvious. Differences in concentration of a given reinforcing agent would be expected to elicit consummatory responses of different vigor. Classical conditioning of these different r_g's to cues in the apparatus would provide different values of K and hence different reaction potential (i.e., E) values for the instrumental response under the different reinforcement conditions. Shifts in concentration of the reinforcer would produce changes in the vigor of the consummatory responses and through transfer changes in the vigor of the conditioned fractional anticipatory goal response and thus the value of K.

Again, it should be noted, no data bearing directly on our assumption that the vigor of the consummatory response varies with the different concentrations of the reinforcing agent were available in these studies. While the response cycle in the free-responding Skinner box includes the time required for the consummatory act, the method does not permit separate measurement of it. Fortunately, some experiments recently carried out by Sheffield and his students have provided the kind of evidence

Fig. 43. Mean rate of bar pressing under aperiodic reinforcement (1-min. mean interreinforcement interval) as a function of the log per cent concentration of sucrose and glucose reward solutions. All points represent the data of a single group of extensively pretrained rats, each rat tested on all solutions. (Guttman, 1954.)

we have been seeking. In a recent experiment Sheffield, Roby, and Campbell (1954) measured the speed of running of rats in a runway as a function of the sweetness, nourishment, and prior opportunity to practice ingestion of the reinforcing agent. In
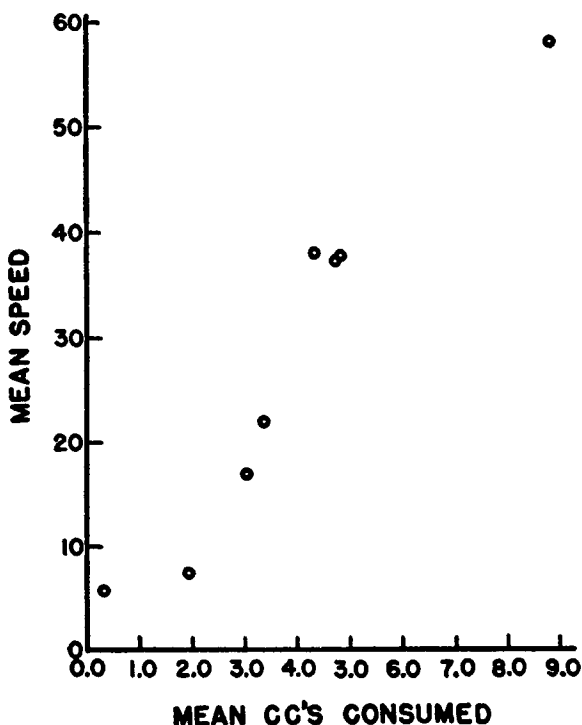
Fig. 44. The relation between the strength of the consummatory response (mean cc. ingested in a 4-min. consummatory period) and the strength of the instrumental response (running speed). Each point represents the data of different groups trained on varying sweetness and nourishment values of the reward solution and given different prior opportunities to practice ingestion. (Sheffield, Roby, and Campbell, 1954.)

addition to measuring the speed of running in the alley they also measured the rate of consummation of the reward in the goal box. Figure 44 reveals that the hypothesis here proposed is supported in that the mean response speed was definitely re-

lated to the speed or vigor of the consummatory response. In addition to this study Sheffield and Roby (1950) have also shown that performance and speed of running in a simple T maze were positively related to the rate of ingestion of the reinforcing agent. Finally, in a study (Sheffield, Wulff, and Backer, 1951) which employed the sex drive and opportunity for copulation it was found that the mean speed of traversing a straight runway was positively related to the strength of the copulatory activity of the rats. According to the theory here proposed these different strengths of consummatory activity reflect the strength of the conditioned fractional anticipatory goal response and thus provide an index of the value of K in determining the reaction potential of the instrumental running response.

Further tests of this incentive motivation hypothesis are currently being carried out in our laboratory. These studies make use of the implication of the theory that the strength of the K factor, based as it is on the classical conditioned goal response, can be manipulated independently of the instrumental running response. Thus in one experiment now in progress two experimental groups of rats, A and B, are being run in a straight alley four feet long to an empty goal box. A third control group (C) is being run to food. At the end of a given number of such trials, one of the experimental groups (A) is given a number of direct placements in the goal box, which is highly similar in its physical properties to the runway, and allowed to eat as much food as the control group (C) had in its training trials. The second experimental group (B) is given a similar number of direct feedings in a box which is very different in its physical properties from those of the runway and goal box. According to our hypothesis the following predictions, among others, may be made concerning performance on the first test trial: (1) that the performance of experimental group A, that fed in the goal box, should significantly exceed its training level; (2) that the performance of this group should exceed that of experimental group B which is fed in the box dissimilar from the goal box.

The basis of these derivations is that through generalization the runway cues will evoke on the test trial the conditioned fractional anticipatory goal response in experimental group A,

and hence the K factor will be much greater for this group than in the training trials. Since generalization is greater in the case of group A than group B, the K factor should be greater in group A and hence the subjects of this group should exhibit the greater speed of running on the test trials.

PERFORMANCE AS A FUNCTION OF DELAY OF REINFORCEMENT

Two Types of Delay of Reward. In turning to the second of the two experimental variables concerned with the reinforcing event, namely the amount of its delay, it is important first to clarify two very different experimental methods of producing a delay which have not until recently been clearly distinguished. The first of these variations, designated by Hull (1952) as the within-chain case of delayed reinforcement, involves instrumental response chains of different lengths and hence different distances between the initial portion of the chain and the reinforcing event at the end of the chain. A good example of this class of delayed reward experiments is that represented by straight runways of different lengths. Still another example is the simple T-maze situation in which one pathway at the choice point leads to a goal box by a short route and the other by a long route. The experimental evidence from both of these kinds of experimental situations indicates that the initial S-R component of the shorter chain becomes with training stronger than the initial member of the longer chain. Thus it has been found that animals can learn to choose the shorter of the two paths leading to the goal (cf. Grice, 1942), while Hull (1934b) has shown that speed of running is faster in the first section of a short runway than in the corresponding section of a long runway. It was this type of delayed-reward situation that led Hull (1932) to formulate the original goal gradient hypothesis.

The second type of delay-of-reinforcement experiment, referred to by Hull (1952) as the nonchaining case of delay, does not involve response chains differing in length but rather has involved the introduction of a period of delay at the end of the response chain, i.e., at the point where the reinforcer is introduced. Thus in an experiment by Perin (1943) using the

Skinner box, the lever was withdrawn after it had been pressed by the subject and the food was then delivered after different periods of delay. In our own laboratory we have employed a similar procedure in a combined runway-Skinner-box situation in which the subject runs from a starting box and touches a bar
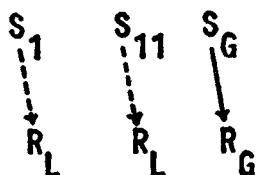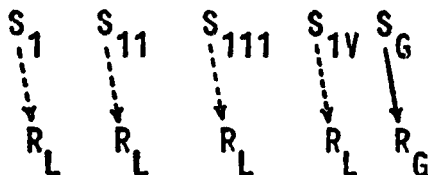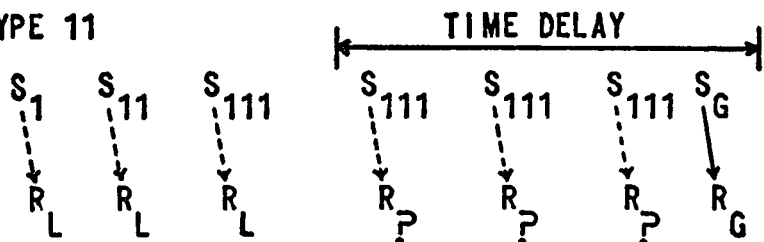
Fig. 45. Diagrammatic representation of two kinds of delayed-reward instrumental conditioning situations. *Type I:* delay is manipulated by varying the length of the measured response chain. *Type II:* delay is manipulated by varying the time interval between the occurrence of the last member of the measured response chain and the presentation of the goal event. R? represent the fact that in these situations the subject is free to elicit responses other than those in the instrumental act.

at the end of the runway (Logan, 1952; Ramond, 1954a). Various periods of delay of the reinforcement are introduced following the touching of the bar, which is withdrawn from the apparatus. In this type of situation, then, delay of reward involves introducing a period of time after the response had been performed during which the subject must wait for the food to be delivered.

Figure 45 summarizes the differences between these two kinds of delayed-reward situations. In the upper portion of the figure are shown two response chains of different lengths which involve different delays of reinforcement of their initial members (e.g., S_I-R_L). The lower portion of the figure represents the second type of delayed-reinforcement experiment. In this type of situation the response chain *that is to be learned and which is measured* under different delays is always of the same length, but a differential period of delay is introduced after the final act of the measured response sequence. During this delay period the subject remains in the situation and is free to and usually does make other responses, including responses that may be incompatible with those in the to-be-learned response chain.

Length of Response Chain as Determiner of Performance. It is my present belief that these two kinds of delayed-reward situations are very different in the sense that quite different factors and hence different laws underlie behavior in them. The first type, that involving different lengths of the response chain, was interpreted by Hull in his *Principles* in terms of a difference in habit strength. That is, the difference in response strengths at the beginning of behavior chains of different lengths was interpreted as due to the development of differences in the habit strengths of these initial S-R tendencies during the course of learning. According to this treatment the maximum habit strength attainable with training was assumed, other things equal, to be a negative exponential function of the amount of the delay of reward. The extent of this gradient in time or distance was further assumed to be a function of the degree to which conditions were favorable for the development of secondary reinforcement. That is, reinforcement of responses beyond the range of a basic or primary gradient was accounted for in terms of the acquisition of secondary reinforcing properties by stimulus cues more and more remote from the primary source of reinforcement.

In a subsequent article (Spence, 1947) I made the further suggestion that this latter aspect of Hull's theory could be modified to assume that all delay of reinforcement involves *immediate secondary reinforcement* which develops in the situation as a function of generalization. Like Hull's original formu-

lation this modified conception also accounted for differences in the strengths of S-R tendencies at varying distances from the reinforced end of a behavior chain in terms of differences in their habit strengths.

Since the development of the concept of the fractional anticipatory goal response as an incentive motivator and its application to instrumental behavior, however, I have favored a quite different conception of how this variable, distance from the reinforcement end of the behavior chain, determines response strength. As in the case of magnitude of the reinforcer I would interpret the action of this variable in terms of variation of the incentive motivating factor, K. That is, the longer the chain the less presumably is the similarity of the stimulus cues at the beginning and ends of the chain and hence the less strong will be the generalized r_g at the beginning of the sequence. This theory implies, then, that differences in reaction potential (E) of responses at different distances from the reinforcement end of the behavior chain are due to differences in the K factor rather than to differences in H.

The adoption of the position that the habit strength of these instrumental responses is not related to variations in the property of a reinforcer places me, I think you will agree, in the contiguity camp. I hasten to add two comments. The first is that this position is a highly tentative one, one for which the experimental evidence is as yet only suggestive and certainly not conclusive. The second is that the assumption holds only, it should be noted, for instrumental reward conditioning but not necessarily for classical conditioning. The possibility exists that habit acquisition in the classical type of conditioning is a function of reinforcement. Again the evidence on this question is quite inconclusive. It is interesting to note, however, that *if* one were to adopt a theory that reinforcement (e.g., need reduction) plays a decisive role in the acquisition of the habit strength of classical conditioned responses but not in the case of instrumental conditioned responses one would have a two-factor theory that is exactly the opposite of the well-known two-factor theory espoused by Schlosberg (1937), Skinner (1938), Mowrer (1947), and others. As far as I know the only psychologist who has suggested this inverse two-factor theory

is Tolman. In an article entitled *There Is More Than One Kind of Learning* (1949), Tolman in his usual tentative, half joking fashion suggested that reinforcement (need reduction) would probably turn out to be a major causal variable in the acquisition of cathexes and equivalence beliefs, but he did not believe this to be the case in the acquisition of field expectancies. As far as I have ever been able to understand what was meant by cathexes and equivalence beliefs, they represent essentially what we call classical conditioned responses, whereas field expectancies refer to the kind of learning that we call instrumental conditioning. If I may be permitted to emulate Tolman's half serious vein I am almost ready to ask him to move over and make room for me in this new theoretical camp. I would insist, however, on keeping my own quantitatively defined S-R concepts. Just what label would be applicable to this position I am not sure; S-R expectancy-half reinforcement-half contiguity would be accurate but hardly convenient.

Period of Delay of Reward as Determiner of Performance. But to return now to the second type of delayed-reinforcement experiment, how is the differential performance that results from the different periods of delay in this situation to be explained? In his *Principles of Behavior* Hull made no distinction between the two types of delay experiments, and he applied his *gradient of reinforcement* hypothesis to both of them. Thus he accounted for the results of Perin's study (1943) of delay of reward carried out in his laboratory in terms of habit differences. In his more recent treatment of this experimental variable in *A Behavior System,* Hull abandoned this conception and in its place introduced a new formulation which attempted to deduce the action of this experimental variable from such other postulates of the system as those concerned with the molar stimulus trace, stimulus dynamism, and stimulus generalization. As I have already indicated, in this last formulation Hull clearly distinguished between the two types of delay of reinforcement and treated them in separate corollaries (IIIa and IIIb). However, as to the new theoretical treatment, I must confess I have never been sure of my understanding of it. While it seems, like the present theory concerning K, to involve the notion of a weaker r_g-s_g mechanism the greater the delay of reinforcement,

nevertheless Hull apparently did not think in terms of a motivational factor. Instead he referred to the decreasing strength of the secondary reinforcing action of the r_g-s_g mechanism on the instrumental acts. To me this would mean that the effect of different delays of reinforcement would still be conceived in terms of a difference in the habit strengths of the instrumental responses. Whether or not this is what Hull's theory actually implies I cannot be sure.

Turning to my own interpretation of this second type of delayed-reward situation, I should like first to say that I think it is much more complex than has been realized. If we stop and analyze this type of delayed-reward experiment we see that the critical point is that during the delay period the subject remains in the goal box or in the situation itself but without reward and with either no necessity or no opportunity for making the response that is to be learned. What we need to consider are the implications of this for the two kinds of conditioned response, classical and instrumental, that we conceive to be occurring in such learning. First, with respect to the classical conditioned consummatory response, it would appear that we have a partial reinforcement situation, for goal-box stimuli occur without the food being given. That is, compared with zero delay any finite delay period can be thought of as providing for a partial reinforcement schedule with the proportion of reinforcements being an inverse function of the length of the delay period. It is thus conceivable that the strength of the classical consummatory response and hence K is somewhat dependent on the length of the delay, being weaker the longer the delay period.

However, I believe that a much more potent factor producing lowered performance under delayed reinforcement in this kind of experiment is the fact that the delay period is essentially analogous to experimental extinction. Indeed, experimental extinction may be thought of as the limiting case of delay of reinforcement, that in which the delay interval is infinitely long. From this point of view decrements in performance with delays of reinforcement would be regarded as resulting essentially from the same factor or factors operating in experimental extinction.

Let us consider now what happens as far as the instrumental response is concerned when the reinforcer is omitted in extinction or delayed as in the present type of experiment. Remaining as it does in the situation the animal now makes other responses; thus it may turn away from the locus of the lever, it may turn around in the runway or goal box, it may attempt to jump up to the top of the box, bite at the door, and so on. Occurring as these responses do to essentially the same stimulus components as does the to-be-learned instrumental response, they likewise become conditioned to them. Thus interfering responses are established which, insofar as they tend to occur on a training trial, will have the effect of increasing the time it will take the appropriate response sequence to run off. With immediate reinforcement, on the other hand, these competing responses are not established and hence there would be little or no such interference.

Experimental Study of Factors in Delay of Reward. Unfortunately, we have very little information as to the conditions determining the development of such interfering or inhibitory responses during the delay period. About all we can do at present is attempt to infer the magnitude of their effects from experiments involving differential delay of reward. The most extensive investigation of delay in this type of situation is that of Perin (1943). Using a Skinner box with a discrete trial technique in which the subject ran from a starting box into an alley containing a lever, Perin measured the time it took rats to approach and press the lever. Following the response the lever was withdrawn and after varying periods of delay a pellet of food was delivered. Figure 46 presents the median speed-of-response curves for four different groups of subjects trained with 0, 2, 5, and 10 seconds' delay. Perin presented his data in terms of latency measures, and he concluded that the asymptotes of performance under the different delays were different. The speed measures also suggest this same conclusion, although it is not so evident from these measures that the asymptotes had been reached.

Some further evidence that speed curves under different delays of reinforcement reach different asymptotes is provided by a recent experiment reported by Ramond (1954a). Ramond's

apparatus, it will be recalled, required the subjects, hooded rats, to run from a starting box to contact a lever at the end of a one-foot alley. As Figure 47 shows, four groups of subjects were run for a total of 104 trials, two groups under one of two drive conditions, four or 22 hours' privation, and two groups under one of two delay times, one second or five seconds. While there is still a suggestion that the final asymptote may
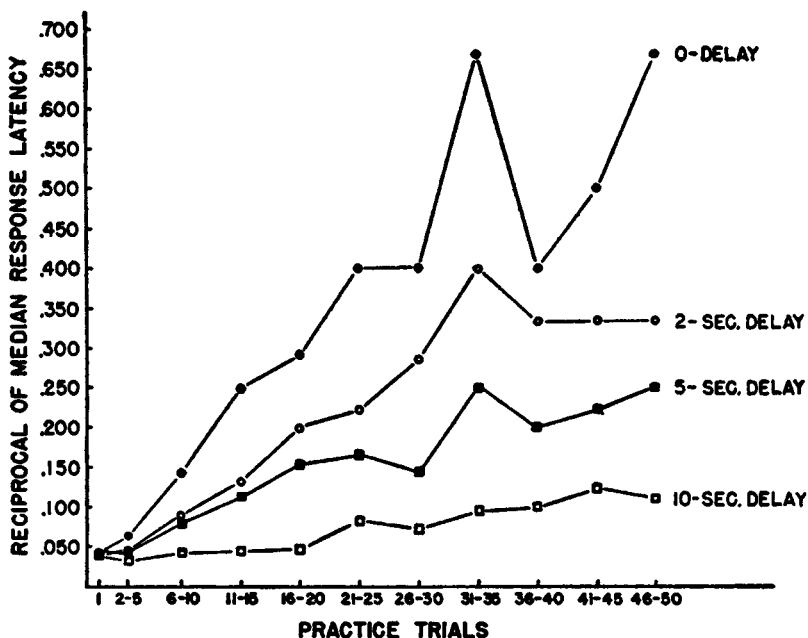
Fig. 46. Acquisition of an instrumental bar-pressing response as a function of delay of reward (type II). The ordinal values are the reciprocals of the median response latencies published by Perin (1943).

not have been reached, the curves for the one- and five-second groups appear definitely to be approaching different final levels under both drive conditions.

Quite in contrast to these results, however, are the findings of an unpublished doctoral investigation conducted in our laboratory several years ago by Harker (1950). As Figure 48 reveals, Harker's group II (10 seconds' control) is still rising long after the one-second control group (I) had reached its

asymptote, and the data suggest the possibility that if training had been continued group II would have attained the same final level of performance as group I. That subjects originally trained on one-second delay for 40 trials and then shifted to 10 seconds' delay were able to continue to respond at a high level is shown in Figure 49. In this graph groups I and III,
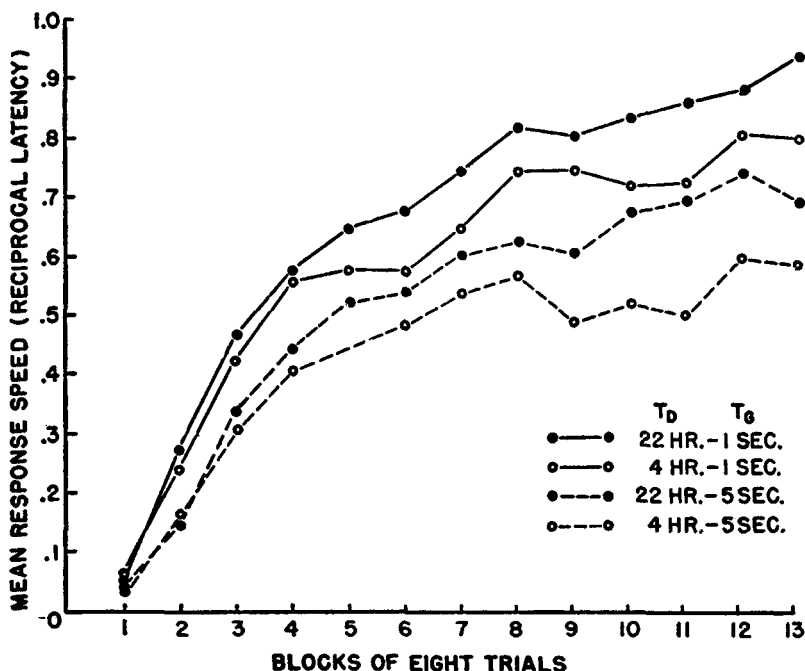
Fig. 47. Speed curves of an instrumental bar-touching response as a joint function of delay of reward (type II) and time of deprivation. The curves of the 1-sec. delay groups apparently reach higher maxima than do those of the 5-sec. group. (Ramond, 1954a.)

represented by open circles, were trained for the first 40 trials with one second's delay. Group I, which is the same group as in the previous graph, was continued as a control on one-second delay. Group III, the experimental group represented by the broken lines, was shifted to 10 seconds' delay. As may be seen, this latter group continued to perform at approximately the same level as the control group (I). Indeed on the last point on

the graph the two curves coincide. It is clear, at least, that subjects that had been trained on a one-second delay were able to respond equally as well when placed subsequently on the longer delay.

One possible factor underlying the difference between Harker's findings and the kind of results that Perin and Ramond obtained is the difference in the type of apparatus used. Harker
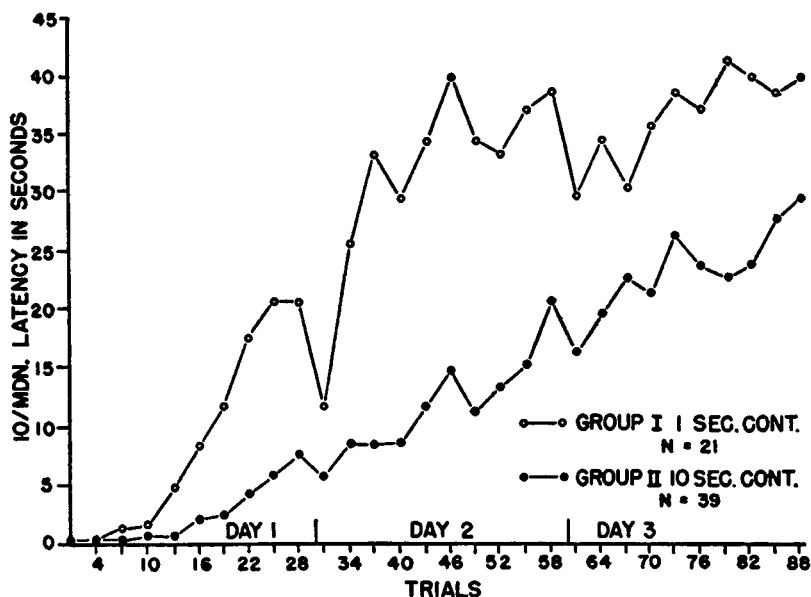
Fig. 48. Speed curves of bar pressing for two groups of rats trained under 1-sec. (group I) and 10-sec. (group II) delays of reward (type II). Note that the effect of delay in this situation is not so much on the performance maximum as on the rate of approaching the performance maximum. (Harker, 1950.)

employed a modified Skinner box in which the response to be learned was the depression of a bar projecting into the response chamber. Unlike the procedure followed in the Perin and Ramond studies, however, the animal was not released on each trial from a starting box and required to run into the response box but instead remained in the response situation at all times. Discrete training trials were provided by the rotation of a bar into the apparatus by the experimenter through a slot in the

front wall. When inserted, the bar protruded into the apparatus immediately alongside the food cup. In responding, the animal was required to press the bar vertically downward about half an inch, at which point it was carried down and out of the box by the action of a spring. Pressing of the bar delivered a pellet of food which the animal could obtain without moving from his
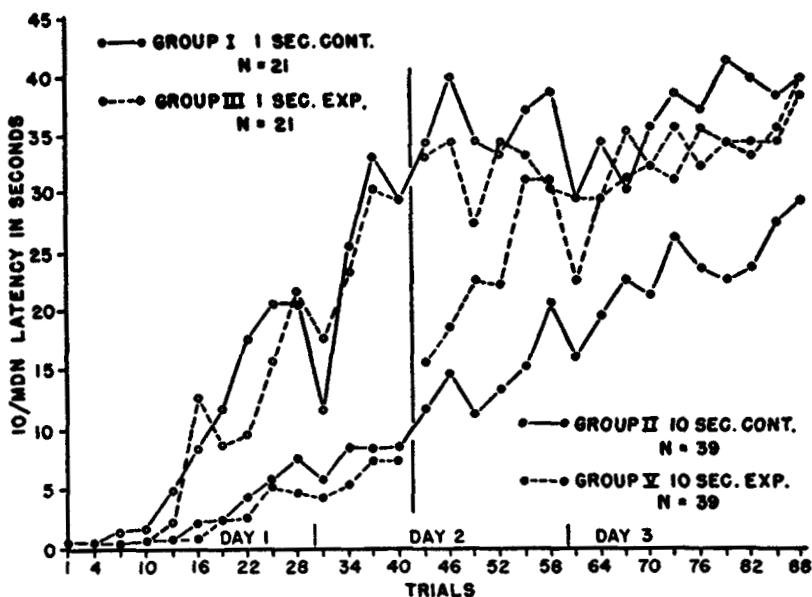
Fig. 49. The effects of reversing the delay of reward on the bar-pressing performance of two groups of rats trained on 1-sec. and 10-sec. times of delay. The dashed curves represent the data of the changed groups (III and V); the unbroken curves show the data of unchanged control groups (I and II). The vertical line indicates the point of reversal for the experimental groups. (Harker, 1950.)

response position. The training trials were given every 90 seconds.

During the conduct of the experiment Harker reported that his subjects, including those run under the delayed condition, developed a marked tendency to remain in a fixed position at the food cup even between trials as well as during the delay period. At the time we did not see the significance of this observation.

Subsequent to this experiment a master's thesis was carried out by Shilling (1951) in which the effect of shifting from a one-second to a five-second delay of reward was studied in the same apparatus that Ramond used. Figure 50 presents the results of this study. The three curves to the left of the graph
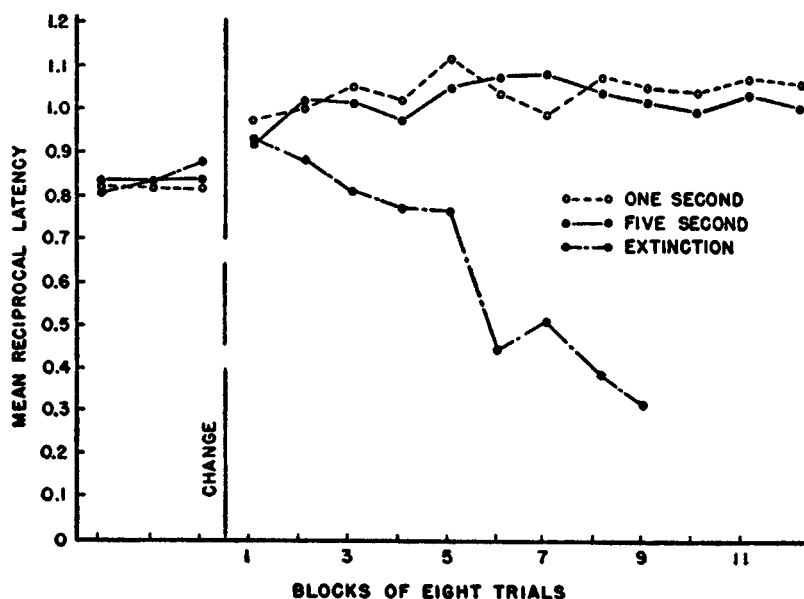
Fig. 50. The effects of increasing the delay of reward (type II) on the performance of a bar-touching response. The curves to the left of the vertical line indicating the point of change depict the final phase of acquisition under 1-sec. delay for three matched groups of rats. The curves to the right of the vertical line show the effects of increasing the time of delay to 5 secs. and to an indefinitely long delay (extinction) relative to the performance of an unchanged 1-sec. control. (Shilling, 1951.)

portray the speed of response for three matched groups on three blocks of eight trials each between training trials 81 and 104. Subsequent to the vertical line indicating a change in procedure, the control group was continued with one-second delay, one group was shifted to five-second delay, and one was given extinction, i.e., no reinforcement. The important point to observe is the fact that the curve for the group shifted to five

seconds' delay did not drop but continued at a high level not significantly different from the control, one-second group. On the other hand when the control group was subsequently shifted to a 10-second delay a gradual decrease in speed of performance to a significantly lower level did occur in approximately 50 trials.

Of major interest here, particularly in relation to the description of behavior given by Harker of his subjects, is the following observation reported by Shilling:

> An interesting observation of the present experiment was that of the marked difference in behavior between those animals changed from one to five second delay and those animals changed from one to ten second delay. Whereas the five second animals almost invariably remained at the food cup during the delay interval making characteristic anticipatory movements, the ten second animals after making anticipatory movements during the initial part of their delay interval tended to turn away from the food cup at some time late in the delay interval (1951, p. 25).

The theoretical implication of these observations of Harker and Shilling is, I think, obvious. They suggest the hypothesis that the decrement in performance with delay of reinforcement is contingent, in part at least, on whether the subject maintains an orientation toward the stimulus complex of the response manipulandum and food box during the delay period or whether it gives up this response adjustment and substitutes other, conflicting ones, such as turning away. Their findings suggest that there is a limit to which this orientation can be maintained in the rat, which is a function, in part, of the type of situation employed. Thus Harker's subjects successfully maintained the orientation for 10 seconds and performed at a level equal to that of subjects that had a one-second delay. In the type of situation Shilling employed, on the other hand, the animals were not able to maintain their orientation for 10 seconds but could for five seconds.

In an attempt to test this hypothesis a master's thesis was recently carried out by Carlton (1954) in which the performance of rats in two situations deemed to be differentially con-

ducive to maintaining the appropriate orientation during the delay was compared. The experimental situation, the ground plan of which is shown in Figure 51, consisted of a starting box which led into a short alley six inches in length at the end of which was the same type of response bar and food cup that had been used by Shilling and Ramond. Contact with the bar led to its withdrawal and delivery of the food pellet into the food cup
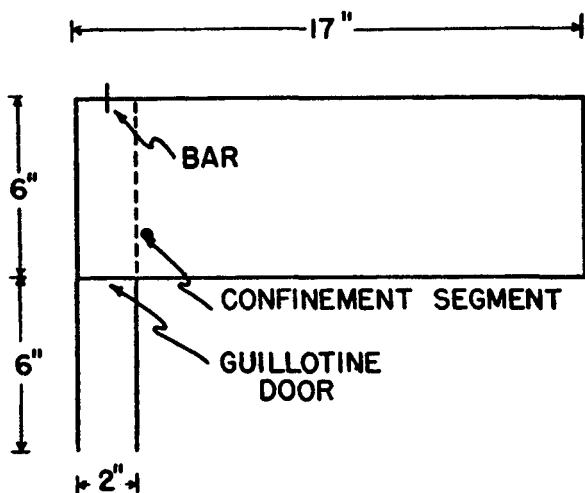
GROUND PLAN

Fig. 51. Ground plan of the apparatus used to investigate the joint action of delay of reward and confinement on instrumental bar-pressing performance. Inserting the inverted-L "confinement segment" into the response chamber reduces both the width and height of the response area. The start box and guillotine door permit the administration of discrete trials. (Carlton, 1954.)

immediately below it. The starting box was two inches in width. The runway used for the confined groups was also two inches in width and had a height of only three inches. It was presumed that these confining conditions would discourage turning away from the food cup during the delay period and thus increase the likelihood of maintaining orientation toward it. The runway employed for the unconfined groups was much larger, being 17 inches wide and 11½ inches high. Such a large response box,

it was believed, would encourage the occurrence of responses incompatible with the maintenance of an orientation to the food cup. According to our hypothesis subjects run in the unconfined compartment should exhibit a lower level of performance than the confined group under a delayed-reward condition but not when the reinforcement is immediate.
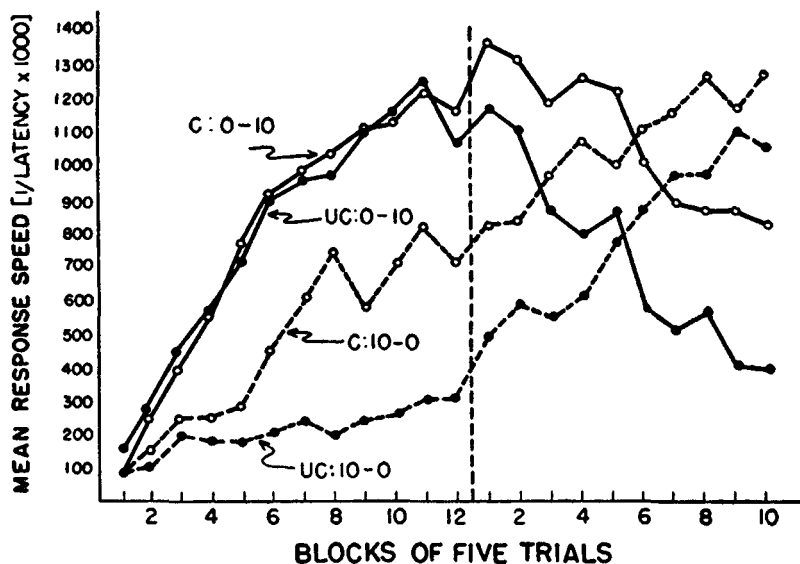
Fig. 52. Bar-pressing performance as a joint function of delay of reward and confinement. To the left of the dotted line indicating the point of delay reversal are the curves of acquisition for confined (C) and unconfined (UC) groups under 0-sec. and 10-sec. delay of reward. To the right of the dotted line are the curves for these groups after the delay of reward was reversed. (Carlton, 1954.)

The results of the study are presented in Figure 52. Two groups of subjects were run for 60 trials in each of the situations, confined and unconfined. For the first 50 trials one group under each of these conditions was rewarded immediately and one had a delay of 10 seconds. It will be seen that there was no difference in the confined (C: 0–10) and unconfined (UC: 0–10) groups at zero delay during the training period. In the case of the delayed reward, however, there was a marked difference, the confined group (C: 10–0) rising to a significantly higher level of response than the unconfined group (UC: 10–0).

At the end of the 50 trials the delay conditions were reversed, the groups which had been on immediate reward being shifted to 10 seconds' delay and vice versa. Our immediate interest is in the groups that were shifted from zero to 10-second delay. It will be noted that the performance of both the confined and unconfined groups dropped with this shift to levels approximately the same as the groups originally trained under comparable conditions. It will be seen, further, that the confined group decreased more slowly and appeared to be leveling off at a higher point of speed than the unconfined group. Also pertinent to our hypothesis is the additional finding reported by Carlton that in the unconfined group shifted from zero to 10-second delay four subjects that showed a marked tendency to wait at the food cup during the delay period performed at a significantly higher level ($p = .02$) than the remaining six subjects of this group, all of whom consistently moved away from the food cup during the delay.

Figure 53 summarizes our discussion of these two kinds of delayed-reward situations. It shows the relevant intervening variables, K and I_t, and the experimental variables in terms of which they are defined. Thus N_g refers to the number of experiences with the goal object; L_g the length of the response chain; T_g the time of delay of the reinforcement; $N_t > O$ the number of delayed reinforcements; and C the degree of confinement. A question mark has been placed after T_g in the case of the experimental variables determining K because it is not at all clear that this experimental variable is an important determiner of K. The inhibitory intervening variable, I_t, which is assumed to be the same as in the case of experimental extinction (I_n), is postulated as subtracting from E to determine the effective excitatory strength $(\dot{E})$ of the instrumental response. While the basis of this inhibition is assumed to be the competing responses that are established during the delay period or during extinction, I have introduced the molar concept of I_t or I_n to represent their quantitative effects.

According to this theory, it will be seen, a reinforcer is assumed to determine the strength of the effective excitatory potential $(\dot{E})$ of the instrumental response. The action of a reinforcer is not conceived, however, as affecting the learning or associative factor, H, but instead it is assumed to determine

response strength through the incentive motivational factor K and the inhibitory factor I_t. The theory is, then, a reinforcement theory as far as excitatory potential is concerned; that is, the presence or absence of a reinforcer and differences in its properties when it is present do make a difference in the strength of the instrumental response. It is not, however, a re-
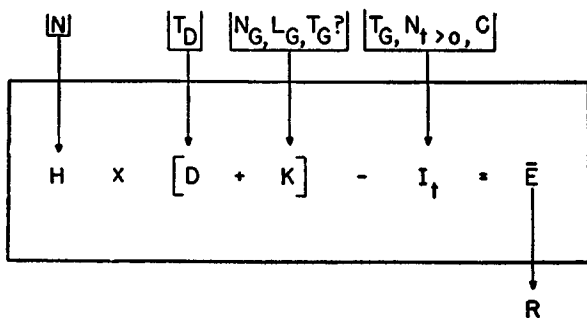
Fig. 53. Diagram summarizing the theoretical treatment of the action of a reinforcing agent. Some of the experimentally manipulable properties of reinforcers are shown at the top of the diagram with arrows leading to the intervening variables, K and I_t, to which they are assumed to contribute. The assumptions made in regard to the manner of combination of these intervening variables in the determination of effective excitatory potential $(\bar{E})$, and hence performance, are shown inside the rectangle. For definition of symbols and further explanation see text.

inforcement theory in the traditional sense of the term, for the habit or associative factor is not assumed to vary with the variations in reinforcement.

Finally, attention should be called to the fact that this treatment of the role of reinforcement in instrumental conditioning has been concerned only with reward learning and not with escape conditioning. We have little or no knowledge concerning the effects of varying the reinforcement (escape from a noxious stimulus) in the latter type of situation; hence there has been little basis for formulating any theory concerning it. My present guess is that it will turn out to be quite different from instrumental reward conditioning.

6. The Role of Motivation in Conditioning

THEORETICAL MOTIVATIONAL CONCEPTS

THE ROLE of motivational variables has been discussed in a number of different connections earlier in the book. Thus two classes of such variables, appetitional and aversive, were briefly identified in our analysis of the factors operating in conditioning and selective learning situations. Again the theoretical concept, general drive level or D, was introduced as being a function of these two classes of motivational variables, and evidence was presented in Chapter 3 to support the notion that the experimental variables determining D combine in a multiplicative fashion with those determining the learning factor, H, to determine response strength. The present chapter will be devoted to an elaboration of the role of drive and other motivational concepts in our theoretical structure. Partly for the reason that our space is limited and partly because we are interested only in trends rather than in specific mathematical functions we shall not attempt to present as detailed a picture of the experimental evidence in this area as was done in the case of the learning factor.

It will also be convenient to reverse the procedure followed in our treatment of conditioning curves and begin with a brief introduction and clarification of the main theoretical concepts to be employed prior to the examination of the relevant experimental data. Figure 54 presents these concepts and shows the interrelations assumed to exist among them. As was mentioned in Chapter 2 the primary or unlearned motivational conditions employed in these experiments fall into two classes which define two types of needs or drive states: (1) appetitional needs and (2) aversive or emotional drive states. These primary motivational states are defined, operationally, in terms of different kinds of antecedent conditions in the environment that are under the control of the experimenter. Thus appetitional needs are specified in terms of maintenance schedules or periods of

deprivation of various kinds of environmental objects required to maintain life or the species, e.g., food, water, sex object. Primary emotional drive states are defined in terms of the administration of some noxious or aversive form of environmental stimulation, e.g., electric shock, air puff, heat, or as in the case of the distance receptors, very intense values of light and sound stimuli.

Each of the appetitional needs, it will be assumed, excites interoceptors producing a characteristic internal, afferent process which will be referred to as a drive stimulus (s_D). Like afferent processes resulting from external sources of stimulation, these drive-stimulus processes are assumed to be able, under ap-

ANTECEDENT CONDITIONS	NEED OR DRIVE STATE
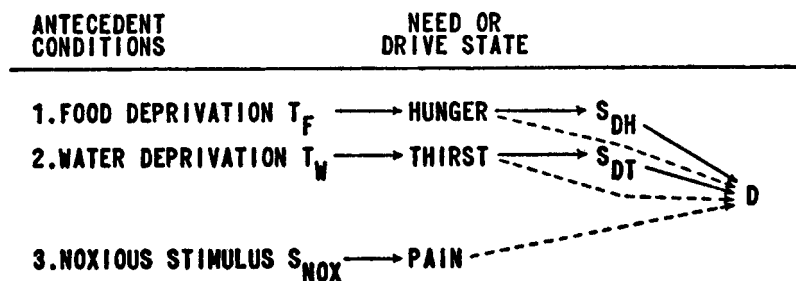
Fig. 54. Diagrammatic representation of motivation theory: the assumptions relating the motivational constructs to their experimental determinants and to each other. The convergence of the arrows to the right of the diagram on the symbol D represents the assumption that all needs contribute to the general drive level. For further explanation see text.

propriate conditions, to acquire habit strength for responses. Further, the several needs or drive states are assumed to contribute singly and in combination to the organism's general drive level (D). This concept of D is similar to that introduced by Hull (1943), and we shall make use of a number of the specific postulates he proposed concerning it.

RESPONSE STRENGTH AS A FUNCTION OF
CONDITIONS PRODUCING PRIMARY
APPETITIONAL NEEDS

Turning now to the experimental investigations of the role of motivation in conditioning, we shall begin with the studies that

have been concerned with the measurement of response strength as a function of the conditions producing primary appetitional needs. Experiments concerned with this problem have been conducted in a number of different ways depending upon the primary interest of the investigators. In one type of experiment (type A) all subjects are first trained under the same period of deprivation (T_d). Subsequently, the subjects are separated into several groups each of which is tested under different deprivation periods. The investigators who have conducted their experiments in this manner have been primarily interested in providing a measure of drive strength, and the empirical relation $R = f(T_d)$ has been taken as reflecting the variation of drive strength with time of deprivation. The classical obstruction-box studies of Warden and his associates (Warden, 1931) and the more recent investigations of Kimble (1951) and Horenstein (1951) involving the use of some form of simple instrumental conditioning fall into this group.

Unfortunately this experimental design makes it impossible to distinguish between the effects of the change in the experimental conditions between the training and test trials per se and the differential effects of the test deprivation conditions. Thus in an experiment in which the original training involved 24-hour deprivation, subsequent comparisons of response strength at different deprivation periods, e.g., 3, 6, 12, and 24 hours, do not make proper allowance for the fact that the 3-, 6-, and 12-hour groups are performing at *different* conditions from those employed in the first training period while the 24-hour group has the *same* deprivation conditions. Interpreted in terms of our theoretical constructs H and D, this design provides assurance that a constant amount of H is developed in the groups to be run subsequently under the different deprivation intervals. It does not, however, control for the possibility that a significant part of the stimulus complex that acquires habit strength for the response consists in the internal cues (s_D) from the need state. If these cues are discriminably different for the different deprivation periods, and there is experimental evidence (Bloomberg and Webb, 1949; Jenkins and Hanratty, 1949) to indicate that they are, then the stimulus complex for any groups run under a different deprivation condition in the test period

from that used in the original training period is changed. This would mean that the habit strengths available for the different groups during the test periods would not be constant but would differ from each other in varying amounts that would, according to the principle of stimulus generalization, be a function of the amount of change in the stimulus complex. As far as the empirical laws relating response measures to time of deprivation are concerned, the findings of these studies have probably been distorted because of their failure to control for these differences in generalized habit strength.

A second set of experiments (type B) has trained groups of subjects under different deprivation periods in a first acquisition period, and then in a second test period either measured response strengths of all of the subjects under the same (i.e., a single) deprivation period (Finan, 1940; Strassburger, 1950) or employed a factorial type of experimental design and had the subjects from each initial training condition allocated equally to each of several testing conditions (Kendler, 1945a). This latter design permits statistical evaluation of the effects of training deprivation levels, testing deprivation conditions, and the interaction of training-testing conditions. Investigators employing the experimental designs in this second class of studies have been primarily interested in the question of whether the learning factor (habit strength) is a function of the different values of T_d employed during the first training period. We shall not have time to discuss the findings of this class of experiments in detail. It is sufficient to say here that the evidence strongly suggests that the habit strength of instrumental reward responses is not a function of the level of appetitional needs during acquisition.

In a third type of design (type C), differential drive stimulus generalization is eliminated by having the training and testing deprivation conditions the same for a particular subject, with different groups of subjects being assigned to the several deprivation periods (Yamaguchi, 1951). A variation of this procedure has involved prolonged training of every subject under several drive levels before measuring response strength of each subject under each level used in the original learning (Cotton, 1953). This latter type of design is probably best

fitted for determining the precise nature of the empirical laws relating response to deprivation conditions. It overcomes most effectively the distorting effects of differential drive stimulus generalization.

As far as the evidence concerning the empirical relation of response strength to deprivation period is concerned, the general statement may be made that response strength in instrumental reward conditioning situations has clearly been shown to be an increasing function, up to a point, of the deprivation time for the various need conditions. In the case of food and water deprivation, there appears to be a maximum period beyond which response strength actually decreases. Data obtained in the early Columbia obstruction-box studies (Warden, 1931), in an experiment by Heron and Skinner (1937), and the more recent investigation of Yamaguchi (1951) suggest that maximum response strength is attained in from three to five days of food deprivation and possibly in somewhat less time in the case of thirst. Presumably measurements at the longer periods of privation are complicated by the effects of inanition.

To confine our interest to the briefer periods of privation (i.e., up to 24 hours), a number of recent studies have provided data showing how different response measures in instrumental conditioning situations vary with food deprivation over this period. Figure 55 presents the findings of an investigation by Horenstein (1951) in which she employed an apparatus that required the rat to push open a panel exposed by the raising of a door in order to obtain food. Two measures of instrumental response strength were employed in this situation: (1) resistance to extinction after an initial training period and (2) speed of response following training. In addition to these measures a third index of response strength employed was the gross food intake in a 20-minute period. The response values in the curves of this graph are given in terms of percentage of the maximum value of each response measure, which in each instance occurred at $23\frac{1}{2}$ hours of deprivation.

It is apparent that the relation between response strength and time of food deprivation for this period is a fairly complex one. The curves for all three measures exhibit a sharp rise between zero and two hours, followed by a more gradual increase

to 23½ hours. Other experiments have confirmed the sharp initial increase during the first two hours, while a number of studies have obtained gradually rising curves for the period between two and 24 hours that tend to show the positive ac-
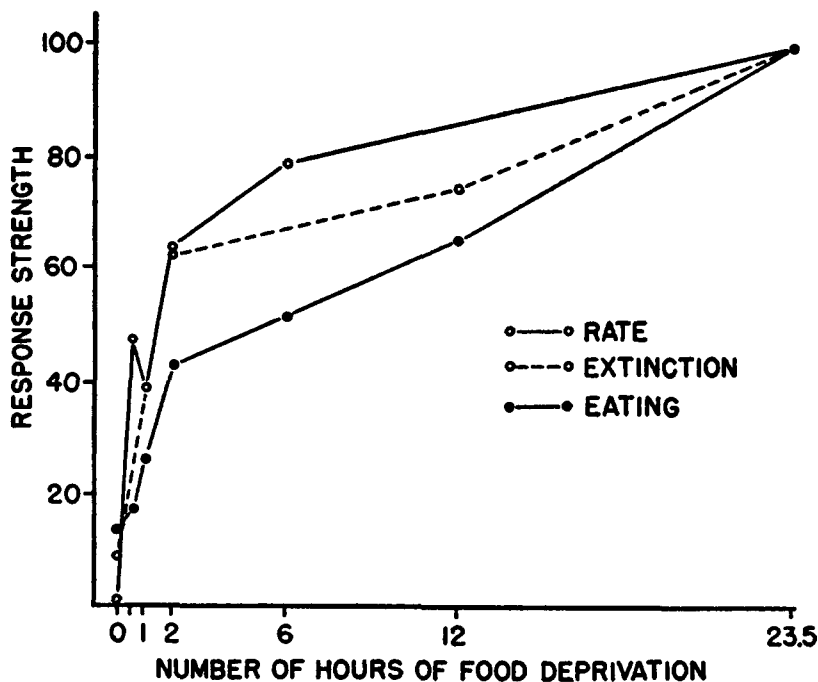
Fig. 55. Three indexes of response strength as functions of the time of food deprivation in the rat. The uppermost and middle curves represent rate-of-responding and resistance-to-extinction measures of an instrumental panel-pushing response; the bottom curve represents the vigor of the consummatory response in terms of amount of food ingested in a 20-min. period. The index of response strength is the percentage of the response value obtained under 23.5 hrs. of deprivation. (Horenstein, 1951.)

celeration exhibited by the two lower curves of Horenstein's study. The extinction data of Horenstein were obtained in a type A experimental design; that is, all of the subjects were trained originally under 23½ hours of food deprivation and then divided into seven groups, each of which was extinguished under a different deprivation period. The other two measures, rate of responding and rate of eating, were obtained in a type

C experimental design in which all subjects were trained under each deprivation period and then tested under each.

While there have been a fairly large number of studies of
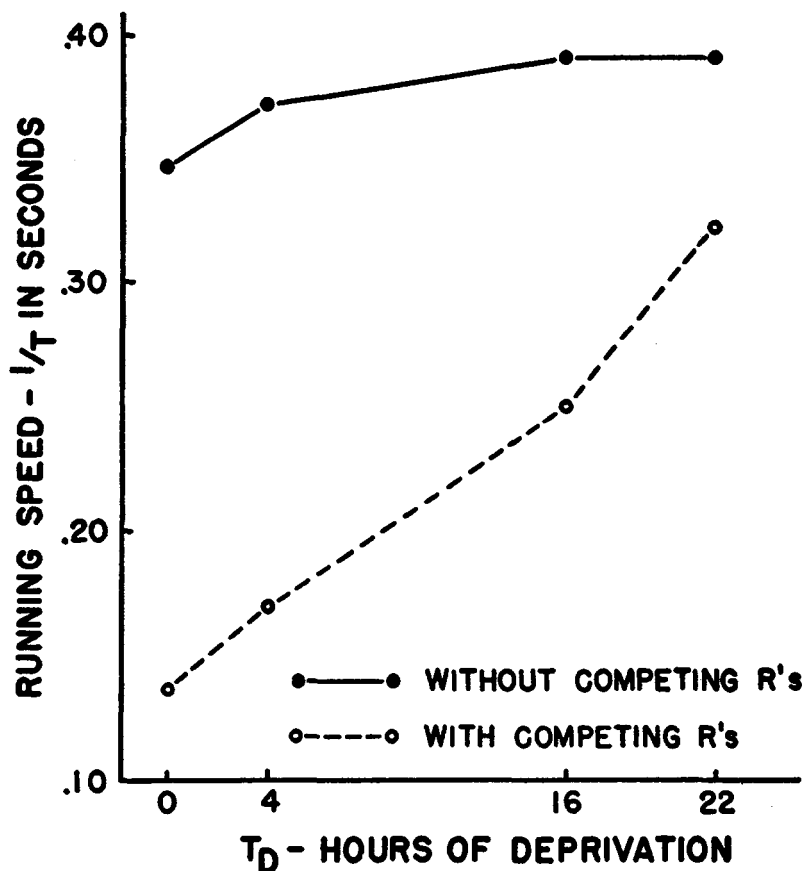
Fig. 56. The effects of competing responses on the relation between running speed and the time of food deprivation. Each point represents the data of the same group of rats trained and tested under all deprivation conditions. The upper curve shows the running speeds for different deprivation times when the data from trials involving competing responses are excluded. The lower curve depicts the same relationship when all data are used. (After Cotton, 1953.)

the motivational conditions underlying instrumental reward conditioning, our knowledge is still in a very unsatisfactory state. A number of recent experiments have raised serious ques-

tions that point to the need for further investigations. For example, Cotton (1953) has recently shown that the function relating speed of running in a straight alley to time of food deprivation was very different for trials that were without competing responses than for trials in which competing responses occurred. As Figure 56 shows there was little difference in running speed under the different deprivation periods when competing responses were not present. Another study conducted by Campbell and Kraeling (1954) has obtained results which suggest that a part of the difference in speed of running under different deprivation periods may be due to the learning of different skills, i.e., animals trained under a high drive may acquire a skill of running rapidly while those trained under a low drive acquire a skill of running slowly. Some recent data involving shifts from one drive level to another, obtained in our laboratory, have also pointed in this direction. We are in dire need of experimental studies in which the performance of subjects trained at a number of different drive levels is subsequently measured at different drive levels.

As far as the classical reward type of conditioning experiment is concerned there is very little experimental evidence on the effects of varying deprivation periods. About all that is known is that after a conditioned salivary response has been established the magnitude of the conditioned response at zero deprivation time ranges from five to 25 per cent of that obtained for a 24-hour deprivation period (Finch, 1938; Zener and McCurdy, 1939).

RESPONSE STRENGTH AS A FUNCTION OF CONDITIONS PRODUCING PRIMARY EMOTIONAL DRIVES

Some experimental evidence with respect to the manner in which response strength varies with changes in the intensity or noxiousness of the source of stimulation in instrumental escape conditioning was presented in Chapter 3 in connection with the discussion of the manner in which this variable combined with the training variable (N). Figures 20 and 21 (pp. 85–6) present the findings of Ketchel (1955) and Campbell and

Kraeling (1953). While both studies show that speed of running was faster the stronger the shock, they gave us little indication as to the nature of the function relating response strength to variations in intensity of the shock. An investigation by Kaplan (1952) of the relation between rate of bar pressing by rats in a Skinner box to escape a bright light re-
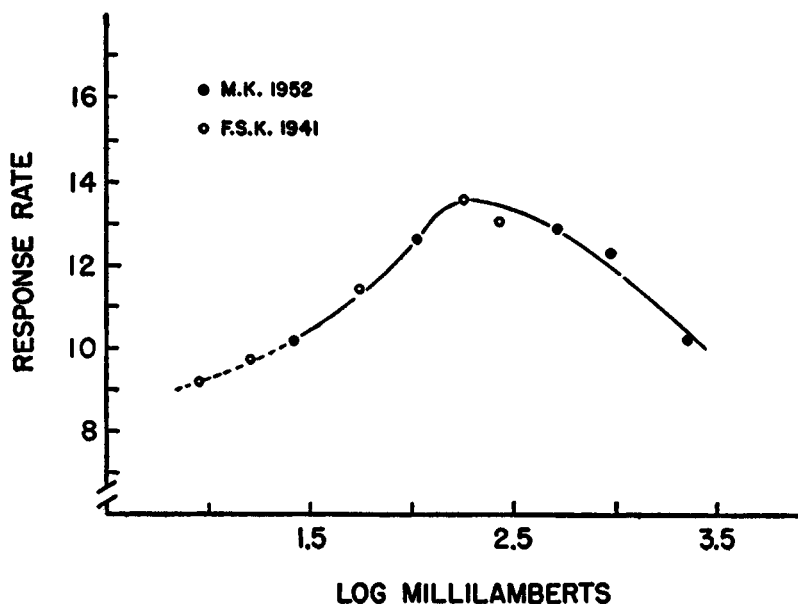
Fig. 57. Mean rate of escape responding as a function of aversive light intensity during intermittent reinforcement. The open circles represent arbitrarily transformed amplitude values recomputed from data published by Keller (1941); the filled circles represent data obtained by Kaplan (1952). The smooth curve was drawn by inspection through the experimental points. (Kaplan, 1952.)

vealed, as shown in Figure 57, an increasing rate of responding up to approximately 180 millilamberts, after which the rate for some reason decreased.

Turning to classical defense conditioning, we recall that evidence was presented earlier which indicated that the level of performance in eyelid conditioning was related to the intensity of the unconditioned stimulus. Figure 58, taken from Passey's

study (1948), shows that the relation between level of eyelid conditioning performance, measured in terms of frequency of conditioned responses in 50 trials, approximates a logarithmic function of the intensity of the air puff. We have also obtained evidence which indicates that a plot of the asymptotic values of frequency curves of eyelid conditioning against the intensity of the air puff provides a negatively accelerated function.
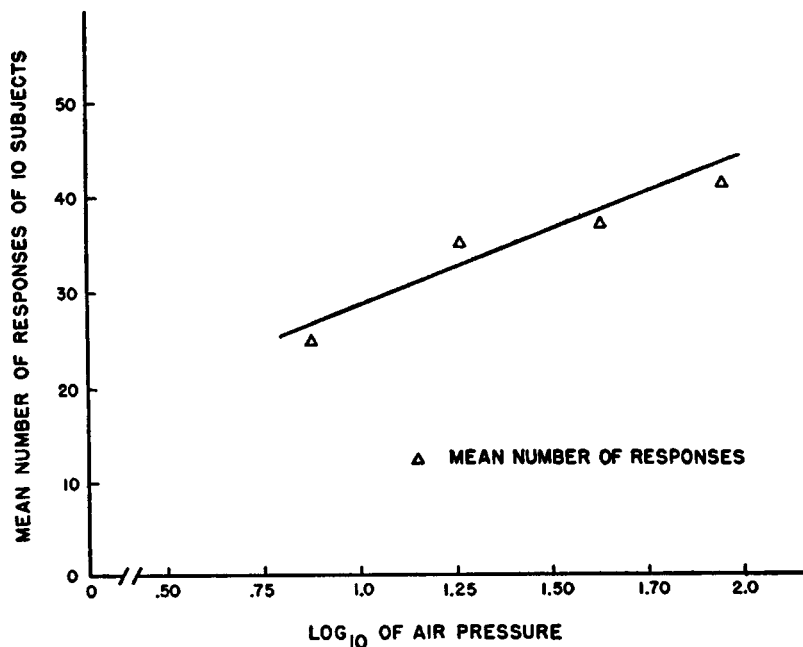
Fig. 58. Mean frequency of eyelid conditioning for different logarithmic intensities of the air puff used as the unconditioned stimulus. (Passey, 1948.)

Whether these performance differences under varying intensities of the noxious unconditioned stimulus reflect, in part, differences in the amount of habit strength developed as the drive reduction-reinforcement theory (Hull, 1943; Miller and Dollard, 1941) would infer or whether they are to be accounted for entirely in terms of differences in the level of D has not as yet been settled. We are at present engaged in a series of investigations which, we hope, will throw further light on this

problem. In the first of these experiments (Spence, 1953a) a factorial design experiment (type B) was used with the conditioned eye-blink as the response. The experiment was conducted in two periods on successive days. In the first period the subjects were divided into two equal groups of 40 subjects. One group was given 30 conditioning trials with a weak unconditioned stimulus (air puff of .25 lb./sq. in.) and the other a like number of trials with a strong unconditioned stimulus (air puff of 5.0 lbs./sq. in.). In the second training period of 20 trials given on the following day, the two groups were each divided into subgroups with half of the subjects from each group continuing with the same unconditioned stimulus and half being switched to the other intensity. The frequency of conditioned responses in the second period provides the data. The values in the four cells of Table I represent the mean number of conditioned responses made by the four subgroups in this period. It will be seen that there are sizable differences in the row and column means. Analysis of variance revealed that both differences were highly significant.

TABLE I

Mean number of conditioned responses in the initial 20 trials of the test phase (day 2) of a factorial design used to investigate the action of the unconditioned stimulus on eyelid conditioning. The differences between both the row and column means are significant, implying the involvement of both associative and motivational factors. See text for further explanation. (From Spence, 1953.)

Day 1 UCS (lbs./sq. in.)	Day 2 UCS (lbs./sq. in.)		Row Means
	.25	5.00	
.25	5.65	8.80	7.23
5.00	7.45	13.00	10.23
Column means	6.55	10.90	

The significant difference between the row means indicates that the differential intensity of the unconditioned stimulus during the first period made for a difference in response strength in the second period. Since the level of intensity of the unconditioned stimulus in the second measuring period was controlled, this difference cannot reflect differences in drive strength (D) resulting from a different unconditioned stimulus

during this period. Some kind of historical factor must thus be invoked to account for the difference. One possible interpretation is that different amounts of habit strength were developed in the first period under the different intensities of the unconditioned stimulus. This interpretation is in line with a reinforcement theory that identifies reinforcement with drive or drive-stimulus reduction and the increment of a reinforcement (H) with the amount of such drive reduction. Identifying drive reduction with the cessation of the unconditioned stimulus and gradations of such drive reduction with the intensity of the unconditioned stimulus employed, it is apparent that the implication of such a hypothesis is that greater amounts of habit strength would be developed with a strong unconditioned stimulus than with a weak unconditioned stimulus.

A possible alternative interpretation of this finding that would not imply differential habit strength is that a stronger emotional or fear response was established in the first training period to the cues from the experimental situation in the case of the more intense unconditioned stimulus. If it is assumed that this conditioned fear response is a determiner of drive level (D), it would follow that subjects trained with the strong unconditioned stimulus would have a higher D level in the second experimental period. According to this formulation the different levels of response are due to this different level of D from the secondary motivation of fear rather than to a difference in H. While this experiment does not permit an evaluation of these rival interpretations, its finding that performance level in the second period is a function of the differential conditions in the first period points to the necessity for invoking some kind of historical or learning factor (differential habit strength or acquired fear response, etc.) to account for it.

The significant difference between the column means of Table I indicates that response strength varied with the intensity of the unconditioned stimulus employed at the time of measurement. Presumably this difference is primarily due to a difference in drive level (D), although it may also, in part, be accounted for in terms of a difference in habit strength (H). An unequivocal answer cannot be obtained from these data, for if habit strength is a function of the unconditioned stimulus then

some of the difference in the column means must be attributed to different amounts of habit strength developed in the second test period. The experimental design necessary to settle this question requires the use of an extinction procedure in the second period rather than continuation of conditioning.[1]

An experiment on eyelid conditioning with human subjects just completed in our laboratory has attempted to equate the drive level during conditioning and yet provide for differential drive reduction.[2] The method employed to accomplish this was as follows. A weak air puff (.33 lb./sq. in.) was given on half of the trials and a strong puff (2.0 lbs./sq. in.) on the other half to two groups. In the case of one group (high reinforcement) the conditioned light stimulus was always paired with the strong puff whereas the weak puff was given without the conditioned stimulus. In the second, low reinforcement group the conditioned stimulus always accompanied the weak puff and the strong puff was always given alone. Thus it will be seen that both groups of subjects received the same average intensity of noxious stimulation throughout the conditioning period; hence the level of drive strength (D) should have been the same for both groups. However, magnitude of drive reduction on a conditioning trial was very different, being much greater in amount for the group (high reinforcement) in which the conditioned stimulus was paired with the strong unconditioned stimulus than in the group (low reinforcement) in which it was paired with the weak unconditioned stimulus.

Figure 59 presents the data for the two groups in terms of

1. At the time this experiment was conducted the only extinction procedure that had been used in classical defense conditioning involved omission of the unconditioned stimulus. Obviously this technique would not have provided the necessary differential drive conditions in the second test period. Since that time a technique that produces extinction but continues the administration of the unconditioned stimulus has been developed in the Iowa laboratory (McAllister, 1953; Spence and Farber, 1953). The procedure involves lengthening the interval between the conditioned and unconditioned stimulus to a period under which conditioning will not occur (e.g., 2,500 msecs.). A difference between the column means under such a procedure would signify a difference in drive (D) exclusively as no further habit growth would occur in this period.

2. This study was conducted as part of a project concerned with the influence of motivation on performance in learning under contract with the Office of Naval Research. The data were collected by Donald Haggard.

the per cent frequency of conditioned responses occurring in successive blocks of 10 paired trials.[3] As may be seen the performance curve of the high reinforcement group gradually but
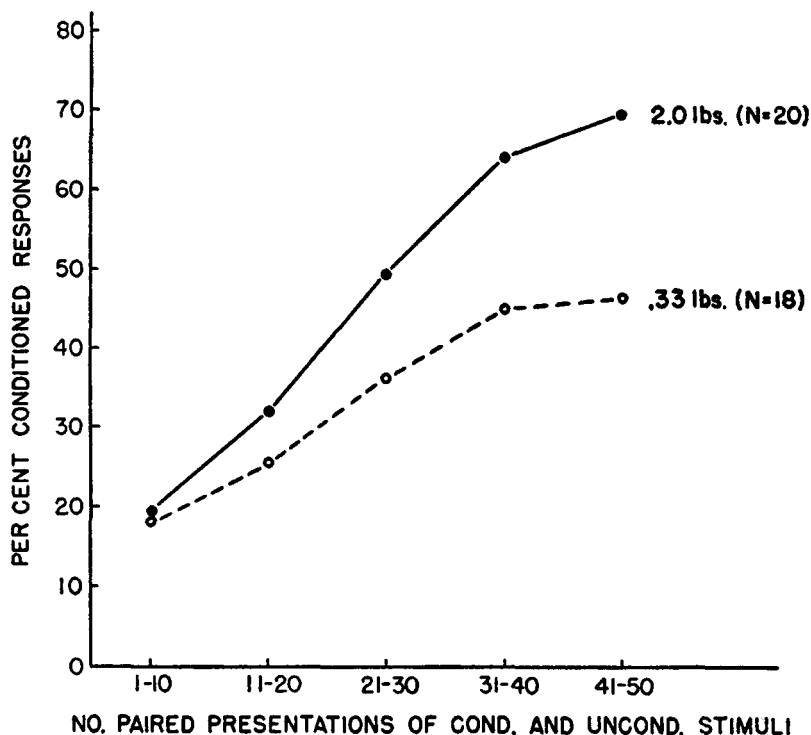
Fig. 59. Frequency curves of eyelid conditioning as a function of the intensity of air puff used as the unconditioned stimulus, with average puff strength held constant. Both groups received 50 presentations of the air puff alone interspersed between 50 conditioning trials. The upper curve presents the data of a group given 2.0-lb. puffs on the conditioning trials and .33-lb. puffs alone; the lower curve shows the data of a group given .33-lb. puffs on the conditioning trials and 2.0-lb. puffs alone.

consistently rises above that of the low reinforcement group with the difference over the last 20 paired trials being significant at the .05 level. This result is clearly in accord with rein-

3. Since the conditioned and unconditioned stimuli were paired on half of the trials, only 50 such pairings were given in the experimental period involving 100 trials.

forcement theory, which would account for it in terms of a habit difference resulting from the differential drive reduction in the two groups on the paired conditioning trials.[4]

Still another study that presents data bearing on this problem is that of Campbell and Kraeling (1953). These investigators compared the speed of running down a gridded alley of four groups of rats under constant shock intensity (400 volts) but with different amounts of shock reduction (to 0, 100, 200, or 400 volts) in the goal box. They found that running speed was significantly related to the magnitude of the shock reduction. This finding, like that of the previous study, lends strong support to the reinforcement theory that performance differences in situations involving variable intensities of aversive stimulation are, in part, a function of differences in habit strength resulting from differential drive reduction. Thus, whereas it would appear that reinforcement theory does not receive much support from studies involving appetitional needs, it does seem to derive considerable support from investigations employing aversive forms of motivation. This is possibly not unrelated to the fact that the experimenter has better control of the drive reduction in this type of situation.

THEORETICAL DRIVE MECHANISM UNDERLYING AVERSIVE MOTIVATION

I should like to turn now to a discussion of a possible theoretical mechanism underlying differences in the general drive level, D, of subjects in experiments involving different degrees of *aversive* stimulation. The hypothesis has been more or less

4. One other possible factor that might account for the findings of this experiment is that there were differences in the amplitudes of the unconditioned responses under the two intensities of the unconditioned stimulus. Contiguity theorists have appealed to this factor although they have never explained how such a difference could account for differences in the *frequency* of conditioned responses. In an attempt to control for this factor in the present experiment two subjects in the weak-puff group who gave small unconditioned responses on the first five conditioning trials were eliminated. Under this condition the mean amplitudes of response for the two groups were approximately equal, the difference being quite insignificant statistically. This problem is receiving further attention in experiments now being conducted in our laboratory.

implicit in the writings of a number of psychologists. My present purpose is to state it more explicitly and to indicate some of the evidence in support of it. The assumption is that the basic mechanism determining the level of D in the case of aversive forms of stimulation is an internal, emotional state or response of the organism (r_e). This emotional response is assumed to be aroused in different degrees with different intensities of such aversive stimuli as shock. Presumably in the case of other afferent channels there are threshold stimulus values above which this response is evoked in varying degrees.

This assumption, that the level of D is a function of the level of some internal emotional response of an organism, has a number of implications for which there would appear to be some supporting evidence. Presumably, as in the case of overtly measured reflexes to various kinds of stimulation (e.g., galvanic skin response to shock, eye-blink to sound, etc.), the hypothetical emotional response (r_e) should exhibit adaptation to continued presentation of the noxious stimulus. Accordingly, after a series of stimulus presentations, the level of drive produced on the occasion of any trial should be lower than if such adaptation trials had not been given. If now a conditioning experiment were conducted *after* such adaptation, the strength of the conditioned response should be lower than if the conditioning were carried out prior to such adaptation.

An experiment conducted precisely according to this design has been reported by MacDonald (1946). The purpose of her study was twofold, (1) to discover if the affective aspect of the unconditioned stimuli employed (air puff to eye and shock to finger) could be adapted out without materially affecting the unconditioned responses and (2) to determine what effect such adaptation to the unconditioned stimulus would have on the formation of a new conditioned response. Concerning the first objective MacDonald found that the galvanic skin response to shock was significantly less after 50 shocks than prior to them and that the spontaneous blinking rate of subjects, which was taken as a measure of emotional tension, increased after five air puffs to the eye and then decreased after 45 more puffs. On the basis of this first part of her experiment, she concluded that the data showed that presentation of the

unconditioned stimulus alone resulted in a marked adaptation of its "affective" or drive-producing function.

In the second part of the experiment MacDonald found, just as would be expected from the present hypothesis, that groups preadapted with 50 presentations of the unconditioned stimulus showed a significantly lower level of conditioned response than groups that were not given such preadaptation trials. Table II shows the percentage of conditioned responses given by the subjects in finger withdrawal and eyelid conditioning under the two experimental conditions.

TABLE II

Mean percentage of conditioned finger withdrawals and conditioned eye-blinks as a function of preadaptation to the unconditioned stimulus. (After Mac-Donald, 1946.)

	Per Cent of CR's in 50 Trials	
Condition	Finger Withdrawal	Eyelid Closure
Adaptation	26.2	24.8
No Adaptation	59.2	63.0

A second implication of this drive theory is based upon the expectation that individuals differ characteristically in the magnitude or strength of their emotional response to the *same* intensity of stimulation. Thus subjects whose emotional response to a particular unconditioned stimulus is strong should, other things equal, have a higher level of D and hence exhibit a higher level of performance in defense conditioning experiments than subjects whose emotional response to the unconditioned stimulus is relatively weak.

A number of years ago we initiated a series of experiments which attempted to manipulate the level of emotionality by selecting subjects who differed in their emotionality or emotional responsiveness. In the first of these studies (Taylor, 1951) a test was developed which differentiated subjects in terms of the degree to which they admitted possessing overt or manifest symptoms of emotionality. In the form of a personality inventory, the test consisted of items which were judged by clinical psychologists to characterize the overt symptoms manifested by anxious, emotional persons. Guided by our theory of the emotional basis of drive, we then set up the hypothesis that

this scale, containing as it did items referring to emotional symptoms, differentiated persons in regard to their level of drive or D. The testing of this hypothesis involved the derivation, with the aid of other portions of our learning theory, of implications concerning differences to be expected in conditioning between subjects who score at extremes of the scale.

Thus, in the case of classical defense conditioning the theory predicted that persons who scored at the high (anxious) end of the scale, having a higher drive level, would exhibit a higher level of performance than persons who scored at the low (nonanxious) end of the test. The derivation that a high D level leads to a high level of response is based, of course, on the assumption that the strength of the conditioned response is a function of excitatory potential (E) which, in turn, is a multiplicative function of H and D.

Figure 8 (p. 65) shows the results obtained in the first of a series of experiments conducted to test this implication of the theoretical assumptions. This graph shows the frequency curves of eyelid conditioning that Taylor (1951) obtained for anxious (upper 12 per cent of distribution of scores) and nonanxious subjects (lower 10 per cent of scores). The difference between the two groups is very striking and highly significant. Since this initial study we have conducted and reported three further experiments that have provided comparisons of anxious and nonanxious subjects in terms of frequency of conditioned eyelid responses (Spence and Taylor, 1951; Spence and Farber, 1953; Spence and Beecroft, 1954). In these studies the upper and lower 20 per cent of the subjects on the anxiety scale were used. Two of these experiments have provided differences in the same direction at better than the .01 level of significance and one at the .05 level.[5]

In the absence of any critical evidence we have entertained two somewhat different conceptions of the nature of the emo-

5. A study by Hilgard, Jones, and Kaplan (1951) which had only 10 subjects per group failed to provide a significant difference between anxious and nonanxious subjects. In two as yet unpublished experiments involving four comparisons between anxious and nonanxious subjects, three of the differences were found to be highly significant. In the single instance in which no difference appeared, a very mild unconditioned stimulus (.25 lb/sq. in.) and a weak conditioned (tone) stimulus were employed.

tional response of these subjects. One is that persons scoring at the high end of the scale react emotionally in a chronic manner to all situations, whether noxious (i.e., threatening) or not, and that they show little or no adaptation of their emotional behavior. The second hypothesis is that high anxious subjects have a lower threshold of emotionality, react with a stronger emotional response and show relatively less adaptation to situations containing a threat or some degree of noxiousness, than do subjects who score at the low end of the scale. Presumably, according to this second hypothesis, extremely mild, nonthreatening situations would not produce a differential emotional response (or D level) in subjects scoring at extremes of the scale.

In an attempt to test these two alternative hypotheses we recently carried out the following experiment with anxious and nonanxious subjects.[6] A very mild air puff (.25 lb./sq. in.) that had previously been shown to produce conditioning and yet was sufficiently mild as to be judged not unpleasant was employed. In addition to anxiety another variable, intensity of the conditioned stimulus, was introduced. Half of each type of subject was conditioned to a very weak tone, one at an intensity level just eight decibels above the ambient noise level in the room. The conditioned tone stimulus for the other half of the subjects was 45 decibels above noise level. This latter tone was reported as being quite unpleasant.

The conception that anxious subjects are chronically more emotional and hence would have a high D level regardless of the degree of noxiousness in the situation would lead to the prediction of a difference in level of conditioning in both of these conditioning situations just as has been obtained in all of our previous studies with stronger air puffs. On the assumption, however, that the weak air puff was not emotion arousing, or at least that all subjects would show ready emotional adaptation to it, the implication of the alternative hypothesis would be that little or no difference in the performance levels of sub-

jects from the extremes of the scale would be found under the condition in which the weak conditioned tone was employed. The strong conditioned tone, on the other hand, would be expected to produce a difference between anxious and nonanxious subjects.

The results of the experiment in terms of the frequency of conditioned responses given in the last 20 trials (61–80) are shown in the upper graph of Figure 60. As may be seen, they were in agreement with the second of our two hypotheses and not in line with the chronic hypothesis. Thus there was no significant difference in the case of the weak conditioned stimulus; indeed the anxious subjects gave somewhat fewer conditioned responses than did the nonanxious. With the very loud tone, on the other hand, the anxious subjects have a significantly greater number of responses.

However, since running this experiment a further study has been conducted in which anxious and nonanxious subjects were conditioned with the same weak unconditioned stimulus (.25 lb. sq. in. puff) and a very weak light stimulus.[7] Quite in contrast to the results of the previous experiment a highly significant difference between anxious and nonanxious subjects was obtained. In fact, as may be seen from the lower graph of Figure 60, the difference was slightly greater than that obtained with a much stronger unconditioned stimulus (1.5 lbs./sq. in.) in the same experiment. Thus, whereas the results of the experiment in the upper graph are contrary to the chronic anxiety hypothesis and in agreement with the second, differential emotional response hypothesis, those in the lower graph do not support this latter theory. At least they require us to assume that even such a mild air puff is emotion arousing.

To return to our theory of the mechanism (r_e) underlying drive in noxious situations, a third respect in which the theory works very nicely is that it provides a satisfactory account of the source of drive in classical defense conditioning. A special problem arises as to the drive underlying the conditioned response in such situations because the response anticipates the
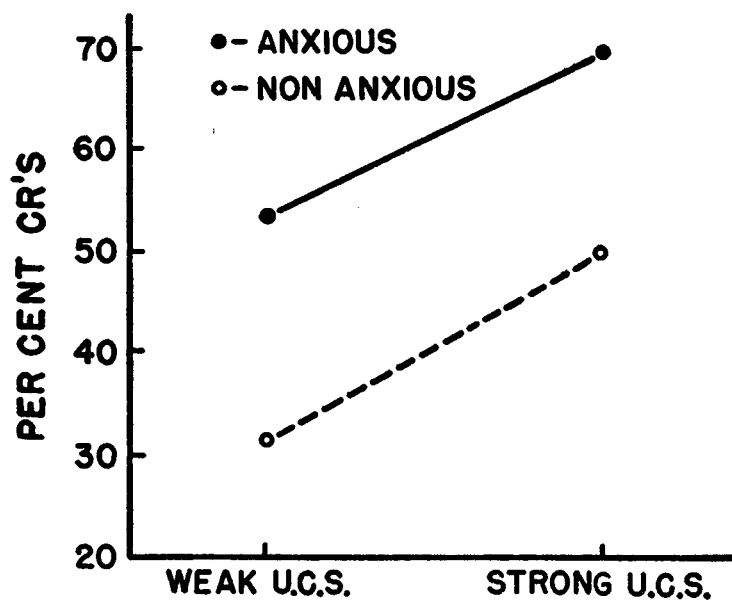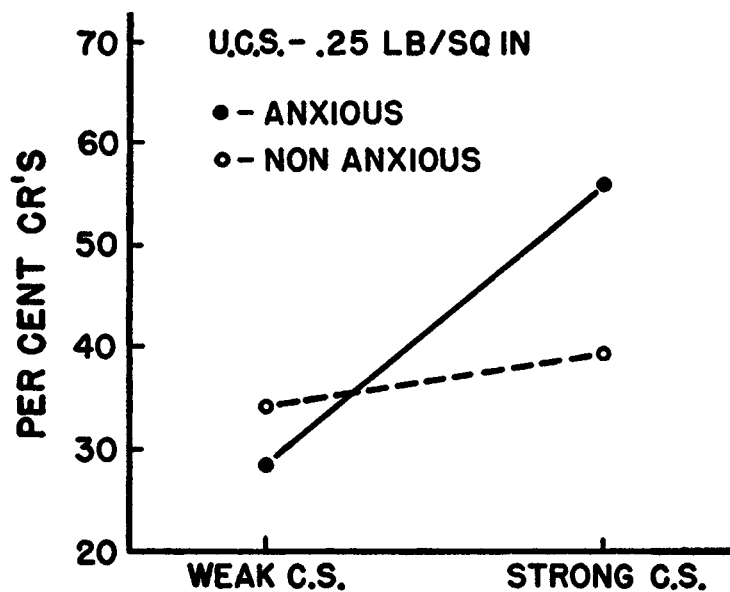
Fig. 60. Eyelid conditioning as a joint function of anxiety level and the intensity of the conditioned stimulus (upper graph) and of anxiety level and the intensity of the unconditioned stimulus (lower graph).

administration of the unconditioned stimulus, which is present only momentarily. This is very different from the situation that exists in instrumental escape conditioning in which the noxious stimulus is present at the time of the response. According to the present interpretation, the drive level operating at the time of the conditioned anticipatory response is a function of the residual effects of the internal emotional response (r_e) to the noxious stimulus of the preceding trials. That is, such emotional responses are assumed to have a relatively persisting effect that extends well beyond the range of temporal intervals usually employed in conditioning experiments. Insofar as the emotional response becomes conditioned to cues in the experimental environment, including in the case of the human subject cues from his own verbal reactions, this would also help to account for the level of D.

Evidence for such persisting motivational effects following noxious stimulation is to be found in the recent studies of the effects upon consummatory responses in animals of administering an electric shock just prior to being placed in the food or water situation (Siegal and Siegal, 1949; Amsel and Maltzman, 1950; Siegal and Brantley, 1951). These studies have shown that the level of consummatory response in a test period following shock significantly increased over the level preceding the shock. This finding was interpreted as reflecting a persisting increment in the generalized drive level of the subjects. Another example in which the response that was increased in strength by a preceding shock was a learned one is that provided by an experiment conducted by Nagaty (1951). After conditioning albino rats to avoid shock by rotating a wheel to the sound of a buzzer, this investigator subsequently tested three subgroups under different extinction conditions: (1) shock given one second before the buzzer; (2) shock given 20 seconds before the buzzer; (3) no shock given before the buzzer. During this extinction period of 50 trials the conditioned response strength of all three groups exhibited a decrement typical of extinction, thus demonstrating that the giving of the shock preceding the conditioned stimulus did not serve to reinforce and maintain response strength. More pertinent to our present interest, however, was the finding that the performance

curves for the two shocked groups were significantly above those of the group not given any shocks. The difference in level of performance of the groups was interpreted by the experimenter as being due to the perseveration of the fear produced by the preceding shocks. The fact that the curves for the one-second group were above those for the 20-second group suggests that there is some kind of gradient effect which is a function of the time since the preceding shock.

Finally, a fourth line of experimental evidence that fits very nicely our emotional response theory of aversive drives are the investigations on secondary or acquired fear drives that had their origin in the work of Miller (1948, 1951). Since these experimental studies are so well known I need only summarize their findings by saying that they have demonstrated, first, that neutral stimuli after association with noxious ones become fear arousing and can serve as the basis for motivating an animal in a learning situation so that it strives to escape from them, and secondly, that reduction of the fear through cessation of the conditioned fear stimulus constitutes a reinforcing event in that it leads to the learning of those responses which it follows.

Closely related to these studies are the investigations of Brown and his students from the Iowa laboratory, which show in a particularly clear manner that the presence of conditioned fear arousing stimuli may intensify coincident stimulus-response tendencies (Brown, Kalish, and Farber, 1951; Kalish, 1954; Murfin, 1954). Thus in one experiment the fear response was conditioned to a combination light-buzzer stimulus. Test trials were also conducted in which this conditioned stimulus was presented in conjunction with an explosive sound that elicited a startle reflex. Figure 61 shows the differential facilitative effect of the conditioned fear stimulus on the startle response in the experimental group as a function of the amount of fear conditioning. A control group also received the neutral stimulus in combination with the shock, but the temporal intervals were such as to prevent the formation of conditioned fear. As may be seen, this group showed no facilitation of the startle reflex in the test trials.

In concluding this discussion of the hypothetical response

mechanism underlying the general drive level D in the case of aversive stimulation, the question may be raised as to whether the same mechanism may not also be operative in the case of
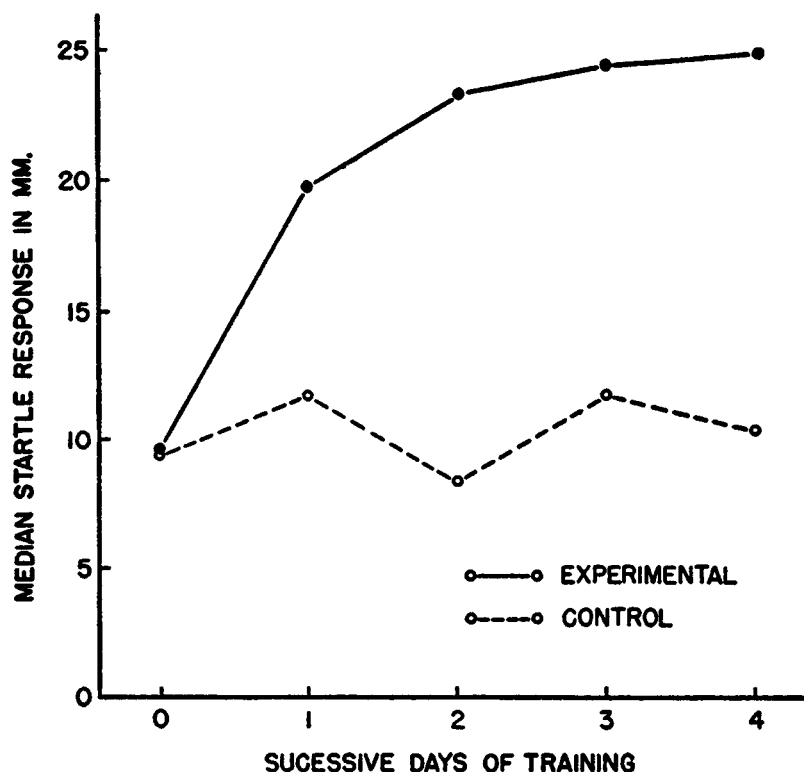
Fig. 61. Conditioning of fear as reflected by the facilitation of an unconditioned startle response with the presentation of a conditioned fear stimulus (light buzzer) just prior to an explosive sound that evoked the startle reflex. The experimental group received paired presentations of a light-buzzer stimulus and shock in a conditioning paradigm. The pseudo conditioning control received the same number of light-buzzer and shock presentations in an unpaired sequence. (Brown, Kalish, and Farber, 1951.)

appetitional needs, the emotional response occurring to different degrees with different levels of the need. I see no reason why the theory cannot be so extended. Indeed it is of some interest to note that such an assumption would provide an explanation

of the difficulties that have been encountered in attempting to establish secondary or acquired motivation in the case of appetitional needs (Myers and Miller, 1954). According to this interpretation the difficulty stems from the fact that the experimenter does not control the occurrence of the emotional response in the case of these needs and hence cannot arrange to present the neutral cues in the temporal relation to it that is required for conditioning to occur.

Finally attention may be directed to the similarity of this hypothetical mechanism to such notions as the central motivational state (c.m.s.) proposed by Morgan (1943). It also fits in with the recent view expressed by Stellar (1954) that the general level of motivation is a direct function of the amount of activity in certain excitatory centers of the hypothalamus.

GENERALIZED DRIVE AS ACTIVATOR OF ALL HABITS

We turn now to a consideration of the interrelations of generalized drive (D) to combinations of needs and to different habit structures. On the basis of an extrapolation of the mathematical equations fitted to Perin and Williams' data relating response strength jointly to deprivation period and amount of training, Hull (1943) was led to infer that if measurements had been made at zero hours' deprivation of food, response strength would not be zero but, depending upon the amount of training, would be some finite value. He interpreted this to mean that other residual needs were present in the organism and that these needs were able to activate the response learned on the basis of the hunger need. Accordingly Hull introduced his construct of *generalized* drive strength or D, and he assumed on the one hand that all of the specific need states contribute to it and on the other that D could energize or activate all habit structures present by virtue of existing stimulus cues regardless of the specific need state under which they were acquired. Thus, a habit that had been acquired under one particular need condition (e.g., hunger) could nevertheless be activated, according to this hypothesis, in the absence of this need, providing the level of generalized D resulting from other needs was greater than zero. As Hull wrote concerning D: It

seems also to have a generalized but possibly weaker tendency to facilitate action of *all* effectors, giving rise to a degree of undifferentiated motivation analogous to the Freudian libido . . . This, together with the assumption that one or more other motivations are active to some degree, explains the continued but limited amount of habitual action of organisms when the motivation on the basis of which the habit was originally set up has presumably become zero (Hull, 1943, p. 252).

Hull further differentiated the need states present in any experiment as either relevant or irrelevant and the strengths they contribute to the total effective drive strength (D) as relevant drive strength $(\acute{D})$ and irrelevant drive strength $(\grave{D})$. This distinction has to do with the needs present in the organism and their relation to the reinforcing agent that the learned response produces. Thus if the subjects in an experimental situation were both hungry and thirsty and the response to be learned, e.g., pressing a bar, brings food (but no water), then hunger would be referred to as the relevant need, thirst as the irrelevant need. That portion of the total or effective drive strength (D) contributed by the hunger need would be referred to as the relevant drive strength $(\acute{D})$, while that portion contributed by the irrelevant need, thirst, would be referred to as the irrelevant drive strength $(\grave{D})$.

At the time of formulation of this theory there was no adequate quantitative evidence relevant to it. Since then a number of investigations bearing on different aspects of the theory have appeared. A study by Webb (1949) was concerned with the basic assumption of the theory, namely that a response conditioned to a particular situation under one need will not only occur in the situation when a new, different need is substituted for the old but will exhibit variations in strength with variations in intensity of the substituted need. Hull expressed this implication in the following corollary: "The number of reinforcements being constant, the stronger an allied but irrelevant drive active at the time of extinction, the greater will be the number of unreinforced evocations required to reduce the reaction potential to a given level, though this number will be

materially less than would be required under the same intensity of the relevant drive" (Hull, 1943, p. 249).

In an apparatus that required white rats to push open a panel to obtain food, Webb first gave all of his subjects a total of 100 training trials under 22 hours' food deprivation. Four groups of subjects were then run in an extinction series under zero hours' food deprivation and 0, 3, 12, or 22 hours respectively of water deprivation. A fifth group (V) was extinguished under the original training motivation, 22 hours' hunger and zero hours' water deprivation. Table III shows the response strengths for the five groups in terms of the mean number of responses made in the extinction series. It is apparent that response strength did vary with different intensities of the irrelevant thirst need as would be predicted from the hypothesis of a general drive factor to which any and all acting needs contribute. Confirmation of this finding has since been reported by Brandauer (1953).[8]

TABLE III

Number of responses to extinction of an instrumental panel-pushing response for four groups trained under 22 hours of hunger and tested under zero hours of hunger and varying degrees of (irrelevant) thirst. Group 5 is a hunger control. (After Webb, 1949.)

Group	Number of Responses	
	Mean	SD
1. H-0 T-0	2.8	1.7
2. H-0 T-3	5.2	2.8
3. H-0 T-12	5.1	2.9
4. H-0 T-22	7.2	3.9
5. H-22 T-0	14.2	7.1

A further finding of theoretical interest in Webb's study is provided by a comparison of the extinction scores of groups IV and V. According to the last portion of the corollary quoted above, response strength under an irrelevant need would be

8. The fact that other studies (Koch and Daniel, 1945; Saltzman and Koch, 1948) have found that response strength was zero when they satiated their subjects for food in a situation in which they had been trained to depress a lever for food does not *necessarily* refute the concept of a nonspecific drive factor (D) that multiplies all existing habit structures to determine response strength. It could merely mean that other sources of drive were not sufficiently strong to produce a superthreshold reaction potential.

expected to be less than that under a relevant need of comparable strength (i.e., where $D = \bar{D}$). If we may assume that 22 hours of water deprivation produces at least as strong a value of drive strength as does 22 hours of food deprivation, we nevertheless would expect the response strength of group IV (irrelevant need of 22 hours of thirst) to be less than that of group V (relevant need of 22 hours' hunger). The table shows that this was actually the case. Group IV required only about 50 per cent as many trials to extinguish as did group V. The basis of the derivation of this finding is the loss of the habit strength provided by the hunger drive stimulus component (S_{HD}) of the total stimulus complex when this latter need is no longer present. That is, the thirst drive stimulus (S_{TD}) was not present during the original conditioning; hence it did not acquire any habit loading for the conditioned response. With the shift from hunger to thirst that portion of the total habit strength contributed by the hunger drive stimulus is lost.

RESPONSE STRENGTH AS A FUNCTION OF COMBINATIONS OF NEEDS

In his theoretical treatment of the manner in which two needs combine to determine the effective drive strength D, Hull (1943) suggested the following rule or "law" of summation:

$$D = 100 \frac{\bar{D} + \underline{D}}{\bar{D} + M_D}$$

where $D =$ effective drive strength resulting from the combined effects of the two acting needs, $\bar{D} =$ drive strength contributed by the relevant need, $\underline{D} =$ drive strength contributed by the irrelevant need, $M_D =$ maximum value of D possible. Conceiving of D as varying on a centigrade scale of 100 units, M_D thus becomes 100 and the coefficient 100 is introduced into the formula so as to maintain the centigrade system. This summation formula was, of course, purely a guess on Hull's part as there were no relevant data available.

A number of experimental studies have since provided some evidence on which to evaluate this initial theoretical attempt of Hull. One investigation by Kendler (1945) has indicated that

the phenomenon of need combination, at least as far as hunger and thirst are concerned, is much more complex than Hull realized. Employing a modified Skinner box involving discrete presentations of the response lever, Kendler compared the response strength of white rats under different combinations of food and water deprivation. With all subjects under 22 hours' food deprivation he systematically varied the water deprivation period for different groups, the periods being 0, 3, 6, 12, and 22 hours. As may be seen from Table IV, which gives the number of trials required to reach the extinction criterion, the response strength of the groups with the additional irrelevant thirst need increased with the increase in the period of water deprivation up to 12 hours. When, however, an irrelevant thirst need based on 22 hours of water deprivation was combined with the 22-hour hunger drive, there resulted an actual decrease in response strength to below that for the condition in which there was only a hunger drive operating. Siegal (1946) has also reported an experiment in which the addition of a thirst drive based on 22 hours' water privation to a 22-hour hunger drive led to slightly but not significantly less resistance to extinction than a group extinguished under the 22-hour hunger drive only. These findings are definitely contrary to the summation formula that Hull suggested, for according to it response strength should have increased in a negatively accelerated fashion to some maximum value with the increasing period of water deprivation.

TABLE IV

Number of responses to extinction of bar pressing for groups trained and tested under 22 hours of relevant hunger need and various degrees of (irrelevant) thirst need. (After Kendler, 1945a.)

Group	Number of Responses	
	Mean	SD
1. T-0	21.4	12.1
2. T-3	21.6	15.5
3. T-6	25.6	15.4
4. T-12	32.9	22.1
5. T-22	18.9	10.3

It should be noted, however, that Hull's rule applies to the values of the relevant drive D and the irrelevant drive $\dot{D}$. Im-

plicit in its application to the present type of problem is the assumption that these values for any given privation period will be the same whether the needs are presented singly or in combination. The findings of Kendler do not support this assumption but instead suggest that the hunger need for any given period of food privation is not so great or does not contribute so much to D when there is simultaneously a need for water as when satiated for water. In other words there is a suggestion that hunger and thirst need states mutually interact in some manner to reduce each other's intensity.

In this latter connection attention may be called to supporting evidence for this interpretation found in the fact that animals without water do not eat as much food as usual, while hungry animals do not drink normal amounts of water. In a recent experiment in which the strength of consummatory responses (eating and drinking) were studied as a function of different types and combinations of maintenance schedules, Verplanck and Hayes (1953) found that albino rats that had been without food and water for 21.5 hours did not consume a significantly greater amount of food in a one-hour measurement period than did animals that had been only without food. Furthermore, these investigators showed that rats that had been hungry and thirsty (again 21.5 hours of deprivation) drank even less water in the hour test period than subjects that had been only thirsty.

How primary and acquired aversive drives combine with an appetitional need (hunger) was the object of an experiment by Amsel (1950a). In the first part of this study Amsel measured the effects on response strength (speed of running in a straight alley) of combining an irrelevant hunger need with a relevant emotional drive to escape shock. He found no evidence for increased speed with the addition of hunger at either a weak or a strong shock level. However, in tests with the same subjects on the following day in which shock was not present and the animals were now running under a conditioned fear drive evidence for drive combination was obtained. Under the acquired fear drive the addition of hunger produced significantly higher running speeds at both levels of acquired fear.

As Amsel has pointed out these findings fit very nicely the Hull drive summation formula, the failure of difference in the case of the shock groups being accounted for on the basis of the fact that the value of the relevant drive was so great that the addition of the irrelevant drive could not produce a very great difference in total drive strength D. At the lower values of the relevant drive provided by the acquired fears the addition of the irrelevant drive from hunger could, on the other hand, lead to a greater increment in the total drive strength.

Earlier some studies on the persisting aftereffects of a prior electric shock on the rate of eating and drinking were briefly mentioned (p. 186). Assuming that an emotional state persists for a period of time following shock we have here a further instance of the addition of an irrelevant drive based on aftereffects of shock to the relevant drive based on hunger or thirst. Supporting the conception that these needs would combine to produce a greater strength of response, these studies found that there was an increased amount of eating and drinking following the shock.[9]

In summary, then, the evidence of these investigations definitely supports the assumption that under some conditions needs do combine to produce greater response strength. However, it would appear that there is no general quantitative law that holds for all need combinations but that, for the time being

9. Attention should be called here to the fact that a number of studies have shown that the addition of an emotional drive to the motivational complex may actually lead to a decrease in the strength of the response in the test situation rather than to an increase as obtained in the above-cited studies. As has been shown, which result will occur depends upon the conditions of introduction of the emotional drive state. If conditioned emotional (fear arousing) stimulus cues are present in the test situation, overt responses (crouching, face washing, escape responses) that compete with the criterion (drinking, eating) response occur. Thus, in the studies in which increased response strength was obtained the shock was administered in a situation quite different from that in which the subjects were tested (Amsel and Maltzman, 1950). In the experiments in which response strength was decreased, on the other hand, the shock was administered in the test situation (Amsel, 1950b; Estes and Skinner, 1951). Amsel and Cole (1953) have shown further that the amount of interference and weakening of the test response depends upon the degree of similarity of the testing situation to the shock situation.

at least, more specific laws will have to be worked out for various combinations of needs.

REACTION POTENTIAL (E) AS A FUNCTION OF THE INTERVENING VARIABLES

In the remaining part of this chapter I should like to discuss further the manner in which the intervening variables are conceived to combine or interact with each other to determine reaction potential and hence response strength. Considerable misunderstanding of this portion of our theorizing is evident. In particular there appears to have been a failure on our part to make it clear that this aspect of the theory, like that involved in the specification of the individual intervening variables, is formulated insofar as it is possible on the basis of experimental findings. As a consequence the mistaken belief seems to have arisen that the specification of the manner in which these intervening variables interact with each other is a matter of pure guesswork.

Apparently this latter notion arose, in part, as a consequence of Hull's treatment of this problem in his *Essentials of Behavior* and *A Behavior System*. Lacking experimental evidence bearing on how some of these variables combined, Hull nevertheless went ahead and speculated freely concerning them. Thus he assumed that the intervening variables V, D, K, and H all combined in a multiplicative manner to determine excitatory potential (i.e., $E = V \times D \times K \times H$). Actually there were data, for example those of Perin (1942) and Williams (1938), from which one could *infer* that D and H combined in a multiplicative manner, while the diverging speed curves of Crespi (1942) and Zeaman (1949) with variation of magnitude of reward suggested that K and H also acted jointly in this manner to determine response strength. However no evidence was available to Hull as to how D, K, and V might combine with each other. In his typical fashion Hull made the multiplicative assumption he did purely as a working hypothesis.

The nature of the problem of determining the manner in which these intervening variables combine may, perhaps, be made clearer by means of the following set of equations:

$$(1) \quad R = f[(fN), (fT_d), (fW_g), (fT_g)]$$
$$(2) \quad R = (fN) \times [(fT_d) + (fW_g)] - (fT_g)$$
$$(3) \quad R = f(E) = H \times (D + K) - I_t$$

where $H = f(N)$; $D = f(T_d)$; $K = f(N, T_d, W_g)$; $I_t = f(N, T_g)$.

One can formulate the task involved in purely empirical terms as that of attempting to discover and formulate the law relating variations in the response to its determining variables. In completed form this law would not only specify the manner in which each single experimental variable determines R but also indicate how they *combine* with each other to determine R. The first equation states the general form of such a law for four experimental variables, N—the number of trials, T_d—the time of deprivation, W_g—the amount of the reinforcer, and T_g—time of delay of the reinforcer. On the basis of manipulation of each variable singly we attempt to ascertain the nature of the several f's shown within each set of brackets. Determination of how the several variables combine with each other requires the carrying out of experiments in which two or more of the variables are manipulated at the same time. While a few such experiments have been reported many more are needed before a reasonably complete specification of the laws of even these simple conditioning experiments will be available.

On the basis of some recent experiments in our laboratory that have involved the joint variation of N and T_g and T_d and T_g, we have tentatively inferred that the law describing the manner in which these four variables combine takes the form shown in the second equation. If now we substitute the intervening variables in terms of the experimental variables which define them, we obtain the third equation, which describes the law in terms of the interaction of the intervening variables.

Let me hasten to add that there still is a considerable element of guessing or theorizing involved in this formulation, for there are often alternative ways of interpreting the empirical findings with respect to how two experimental variables combine in their action. All one can do is make the best inference possible on the basis of the existing data, always being ready

to modify the formulation or consider others as evidence from new investigations requires it.

An example of the use that may be made of experimental evidence involving the manipulation of a number of variables is provided by the experiment of Ramond (Figure 47, p. 156) comparing the performance of white rats in a simple runway as a function of variation of both T_d and T_g. Analysis of variance of these data revealed that there was no significant interaction between the two experimental variables or, in other words, that the differences in performance between the two delay conditions at the different drive levels were not significant. The postulated relation that I_t subtracts from $D \times H$, i.e., E, is in harmony with this finding. If it is assumed that T_g also affects K, the assumption that D and K add is likewise in line with it. Finally, the fact that the performance curves for the two different delay periods (drive held constant) diverge to a maximum also fits in with that part of the theory which assumes that H and K multiply each other. If, on the other hand, we assume that the major effect of variation of T_g is through I_t, then this divergence would have to be accounted for by assuming that I_t is an exponential function of N with its asymptote being some positive function of T_g.

This concludes our discussion of these motivational variables in conditioning. If a complete theory of conditioning phenomena were being attempted in this book a number of other variables, such as the intensity of the conditioned stimulus, the amount of work involved in the response system, the time interval between trials, and so on, would need to be considered. However, as I have indicated, I have selected for treatment only certain problems that have been of interest to us and which we have been investigating. In the next and final chapter we shall be concerned with ascertaining whether the inferences made concerning the nature of the laws governing the joint action of some of these variables from conditioning data will hold up when applied to more complex instances of learning involving competing responses.

7. *Some Applications of the Theoretical Model to Complex Learning*

STATED in its simplest form the method of science is to proceed from observation of particular facts to the establishment of general, quantitative laws from which other particular facts and lower order laws may be derived. In the case of most psychological phenomena the behavior manifestations and their determining conditions are so many, varied, and complex in their interrelations that it is extremely difficult to arrive at such comprehensive laws. In this circumstance psychologists have attempted to facilitate this process by making guesses as to the structure of the laws on the basis of the existing data. Such theorizing typically includes among other things the introduction of abstract concepts (i.e., intervening variables) with their postulated relations with each other and with the experimental variables.

Now there are psychologists who deplore all such theorizing, believing that in the long run theory is a deterrent to progress rather than an aid. While it is unfortunately true that one can point to instances of premature, even harmful, theorizing in psychology, it does not necessarily follow that all theorizing is bad. Precisely specified theoretical concepts that permit the derivation of readily testable experimental implications will lead no one astray. Furthermore, if one may judge from the experimental literature, such theories appear to be highly conducive to motivating experimentalists, including those hostile to theorizing, to conduct and report investigations.

Quite apart from this purely heuristic function, however, theoretical structures involving more abstract concepts are a necessity if we are ever to get beyond the level of a multiplicity of isolated low order laws specific to each different behavior

situation. The major purpose of the theoretical concepts proposed here is not only to provide for the integration of the specific laws found in conditioning experiments but also to attempt to extend in a preliminary way the theoretical structure so developed to more complex behavior situations.

Whether the knowledge we possess today in this field is ready for such attempts at theoretical integration can only be ascertained by trying, for there is no recipe or set of rules that will tell one precisely when any realm of data is ready for such efforts. Undoubtedly differences in the attitudes and interests of individual psychologists will determine their predilection for engaging in such theorizing and hence the point at which they will be likely to try to introduce it. In view of the need and importance of such attempts at theoretical integration in psychology the amount of intolerance, even hostility, that theories appear to arouse is most unfortunate.

In this last chapter I shall attempt to show by some examples the manner in which the theory developed in connection with classical and instrumental conditioning may be extended to somewhat more complex behavior situations. This effort will necessarily have to be somewhat sketchy and incomplete, for neither the theory nor our control of the experimental situations will permit any but the grossest kinds of applications and experimental tests. Again the presentation is directed as much toward highlighting the kinds of problems that arise as to evaluating the worth of the particular theory.

THE SELECTIVE LEARNING SITUATION AND ITS BOUNDARY CONDITIONS

In extending the theory from the single response situation to the multiple, competing response paradigm of selective learning, we shall begin with an extremely simple instance of this kind of learning. Figure 62 represents, diagrammatically, such a simple two-response, selective learning problem. The stimulus situation is assumed to involve two discriminanda, each of which, because of transfer from past experience, has excitatory potentials for evoking receptor-orienting and locomoting responses toward them. These discriminanda are so located in

space, however, that the two responses are incompatible; that is, response to one precludes response to the other. While there are other stimulus aspects of the situation that may be responded to, only responses to the two critical discriminanda are recorded. The response measure employed is the percentage of times one or other of these responses occurs on each trial or block of trials. A concrete example of such selective or competitional learning is the spatial discrimination situation known as the single-choice T maze in which the subject can respond
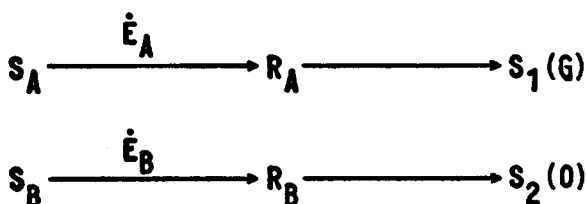
TWO RESPONSE SELECTIVE LEARNING

Fig. 62. Diagrammatic representation of simple, two-response selective learning. S_A and $S_B =$ the two spatially separated (relevant) stimulus cues or discriminanda; $\dot{E}_A$ and $\dot{E}_B =$ the momentary values of the excitatory tendencies between each of the discriminanda and the reactions, R_A and R_B, associated with them; and S_1 and $S_2 =$ the stimuli consequent to the elicitation of R_A and R_B respectively.

by running into either the left or the right alley at the point of choice. Another apparatus that we have employed is a simple problem box in which two levers are placed sufficiently far apart so that only one can be approached and operated at a time.

At this point it is necessary to remind ourselves that the application of a theory to any particular situation involves not only a consideration of the laws or hypothetical relations postulated in the theory but also a careful taking account of the initial and boundary conditions of the situation. That is, the logical implications of a theory are always a joint function of both the laws (hypotheses) and the particular combination of conditions in the situation. If one may judge from the current

psychological literature there is a truly astonishing lack of appreciation of this requirement. For instance, I have long since lost track of how many "disproofs" there have been of my theory of discrimination learning in nonarticulate organisms (Spence, 1936, 1937) that have employed college sophomores as subjects. It is possible, of course, that the difficulty in this instance is merely a semantic one in that these investigators have regarded the term nonarticulate as applicable to college sophomores.

Similarly, many other instances could be cited of failure in the testing of psychological theories either to take into account the boundary conditions in making predictions as to the expected experimental outcome or to satisfy certain conditions specified by the theory as necessary for its testing. A particularly important set of conditions that must be met in the present instance is that both the relevant discriminanda in the spatial discrimination situation must be: (1) effective stimuli from the beginning of selective learning and (2) received at the same time, thus evoking excitatory potentials to the two responses at the same moment. As far as the first of these conditions is concerned the standard preliminary training procedures would appear to provide for its satisfaction. Thus preliminary training of running down a straight runway for food or pressing a single lever will insure that the essential receptor-exposure adjustments will have been acquired prior to the selective learning.

The second requirement, that both discriminanda be received at the same time, stems from a composition law that must necessarily be introduced when applying the theory to the more complex situation involving competing responses. The law that we shall employ is that in such competing response situations the response that has the greater excitatory potential at the moment will be the one that will occur. Hull has expressed this law as follows: "When the reaction potentials to two or more incompatible reactions occur in an organism at the same time, only the reaction whose momentary effective reaction potential is greatest will be evoked" (1943, p. 344). A physical arrangement whereby both discriminanda are in the field of vision at

the point of choice should insure the simultaneous occurrence of the two competing reaction potentials.

These conditions having been satisfied so that the two discriminanda evoke simultaneously competing excitatory potentials, which response will occur on a particular trial will depend, in part, upon the difference between the excitatory strengths of the two responses. Making use of the theory of oscillatory inhibition it may be shown that the momentary excitatory potentials (E_A, E_B) will be distributed according to a normal probability function in which the range and sigma of I_o are equal and constant for each response throughout the learning. On the further assumption that the oscillatory inhibition values for the two responses are asynchronous or uncorrelated, it is possible to determine for any given difference between the excitatory potentials of the two responses the probability that the one which has the greater excitatory strength will occur. Thus, as shown in the following equation,

$$R_p = 100 \int_{-x}^{+\infty} \frac{1}{\sqrt{2\Pi}}\, e^{-x^2/2}\, d(x)$$

the theoretical per cent occurrence of the stronger response may be determined from the normal probability integral in which x, the abscissa value, is equal to the difference between the two competing reaction potentials divided by the sigma of I_o times $\sqrt{2}$.

This determination will hold, however, only as long as one of the distributions of oscillating excitatory potentials remains entirely above the threshold. (Strictly speaking, of course, on the assumption of the normal probability distribution this is never the case, but for all practical purposes we may assume that values beyond 2.5 sigma do not occur.) This condition is met in either section III or IV of the graph in Figure 63. In each of these instances, it will be noted, one of the distributions does not fall below the threshold. However, when both distributions of momentary E values extend below L, as shown in sections I and II, the percentage occurrence of one response over

the other is a function not only of the difference between the competing E values but also of the extent to which the two distributions fall below L. This is most clearly seen by comparing sections I and IV in the graph. The differences between
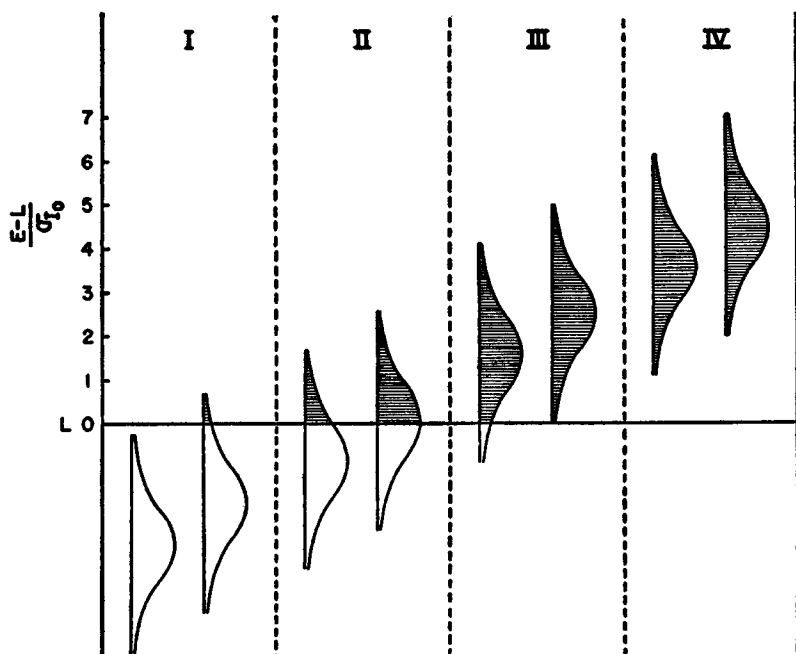
Fig. 63. Diagram depicting four stages in the movement of two competing momentary reaction potential ($\dot{E}$) distributions across the reaction threshold ($L = 0$) with constant difference in reaction potential. As a graphic illustration of the deduction that the percentage occurrence of the competing responses is a function of the absolute level above threshold of the two $\dot{E}$ distributions note that the ratio of the superthreshold (shaded) areas of the two distributions decreases with the upward movement of these distributions (compare especially secs. I and IV).

E_A and E_B are exactly the same in both instances, and yet the percentage occurrence of R_a in section I is 100, while in section IV it is obviously much less than 100.[1]

1. In this connection it should be noted that we assume that the subject remains in the situation until, eventually, variation of the receptor-exposure adjustments produces a value of I_o that is sufficiently small to provide a super-threshold $\dot{E}_A$.

I shall not go into the mathematical details of determining the percentage choice of the responses in cases in which significant portions of both distributions fall below the threshold. The mathematical solution involves the evaluation of a double normal probability integral.[2] For our present purposes it is sufficient to note that, given a constant difference between E_A and E_B, as the absolute strengths of the excitatory potentials are increased the per cent occurrence of R_a will decrease from 100 per cent as in section I of Figure 63 to a minimum which is reached when one of the distributions of momentary E values is, for all practical purposes, entirely above L (section III). Figure 64 presents a picture of how this per cent choice of R_a (shown on the ordinate) varies with the absolute strength of the E values for two different values of $E_A - E_B$ that produce, respectively, 64 and 80.6 per cent choice of R_a beyond the point $(5\sigma_{I_0})$ at which the distribution of momentary E's for the stronger response is entirely above L.

From these considerations it may be seen that the percentage occurrence of the competing responses is a function not only of the magnitude of the difference between the competing E's but also of their absolute level above L. Thus it is possible for a given difference in the competing E's at very low absolute values of E to produce a higher per cent of occurrence of the stronger response than a larger difference at higher absolute levels of E. That this will have considerable significance for comparisons of performance in this type of situation under different levels of drive is readily apparent when we consider that the effect of different drive levels is to vary both the absolute levels of the competing reaction potentials and the magnitude of the difference between them.

Situation Involving Differential Number of Reinforcements. The first selective learning situation that we shall consider is

2. While a mathematical solution of the problem involving some function other than the normal probability integral would apear to be feasible, I have made use of an approximation that employs the normal probability tables. I am indebted to Frank Restle, Human Resources Research Office, George Washington University, for suggesting this method and for help in working it out. Larry Stein also made important contributions to it and prepared Appendix A, which describes the method in detail.

one in which both responses receive reinforcement under identical conditions (i.e., same delay and magnitude of the reinforcer) but the schedule of reinforcements is such that one response receives twice as many reinforcements as the other. An experimental arrangement employed by Ramond (1954b)
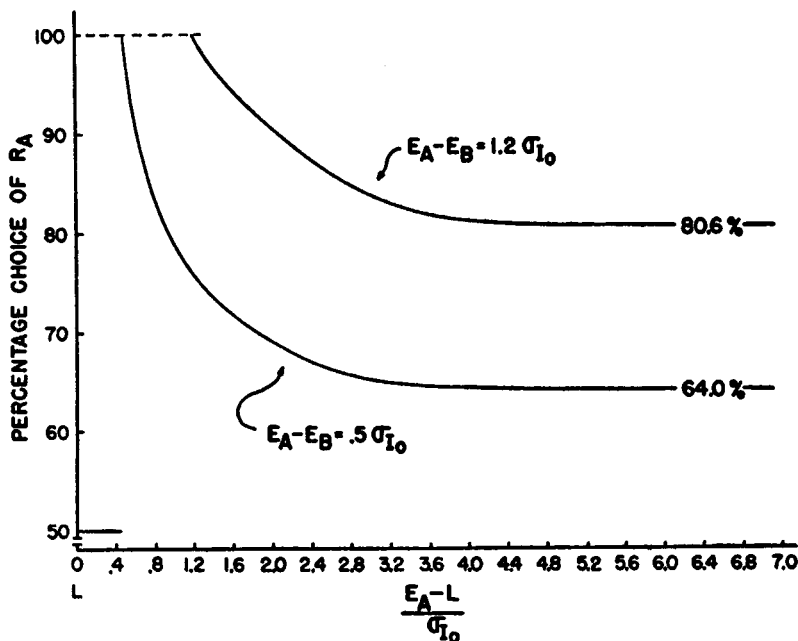
Fig. 64. Percentage choice of the dominant response (R_A) in two-response selective learning as the momentary reaction potential distributions of the competing reactions move across the threshold with a constant difference in reaction potential. The upper curve represents the choice function when the differential reaction potential $= 1.2 \, \sigma_{I_0}$ and the lower curve that when $E_A - E_B = .5 \, \sigma_I$ o. The values on the abscissa indicate the heights of E_A above the threshold in units of the standard deviation of oscillatory inhibition.

in this type of experiment is shown in Figure 65. The apparatus, a double-bar Skinner-type box, permitted the presentation of both bars together or either bar singly. The illumination level in the response chamber was such that the bars and food cups immediately below them were not visible unless the shielded lights above them were lighted. On free or choice trials both lights were illuminated and both bars were available. "Forced"

trials consisted in having only one bar available with the light above it illuminated. On such trials the light on the opposite side was turned off and the food cup was not visible.

18"

SHIELDED 7
WATT BULB

BAR AND
FOOD CUP

BRASS FLOOR

GLASS DOOR
AND
METAL DOOR

STARTING
ALLEY

10"

12"

12"

2 1/2"
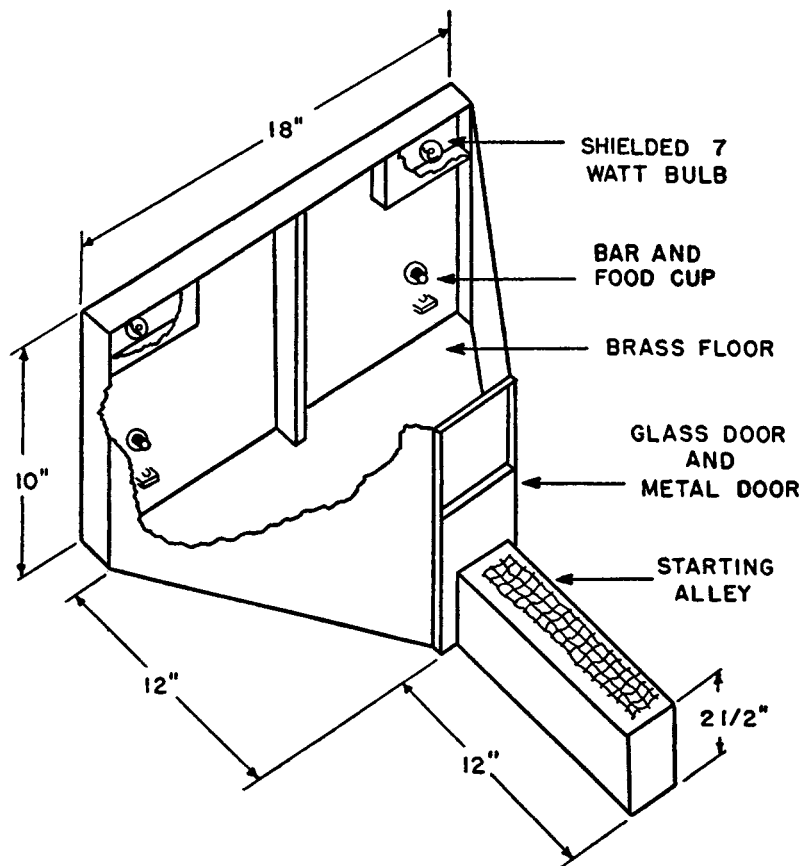
Fig. 65. A cutaway view of the double-bar Skinner-type box. Either or both of the bars can be presented through holes in the rear wall. Raising the glass door starts a clock. By touching a bar, the rat completes a low-ampere contact relay circuit between the brass floor of the box and the bar which stops the clock, retracts the bar(s), and activates the feeding mechanism. (Ramond, 1954b.)

Trials were administered in blocks of three, the first trial being free and the other two forced. The forced trials were arranged so that in any one block two reinforced trials were

given on one of the bars and one on the other bar. As a conse-
quence of this procedure, prior to each free trial after the first,
the subject had had twice as many reinforcements on bar A
as on bar B.

The primary objective of this experimental arrangement was
to provide a selective learning situation in which the develop-
ment of the difference in reaction potential between the two
competing responses would be due to the learning and incentive
factors, H and K, and not to the inhibition factor, I_n, which
must be taken into account when one of the responses is not
reinforced. Since we were interested, among other things, in
testing whether the multiplicative relationship between H and
D inferred from simple conditioning studies would hold up in
the competitive response situation, we were desirous of eliminat-
ing the necessity for taking into consideration the inhibitory
factor.

In Figure 66 is represented the growth of the hypothetical
excitatory potentials of the two responses A and B during the
training. The particular values chosen for the maximum and
rate-of-growth parameters will be discussed later in connection
with their specific application to Ramond's data (1954b). For
the present I should like to call attention to the fact that the
difference between the reaction potentials of the two responses
at first increases until it reaches a maximum, after which it
decreases to zero as the maximum of the two growth curves is
approached. According to the theory, then, we should expect
the per cent choice of bar A to increase to a maximum and then
decrease. Now it is a simple matter to determine, mathemati-
cally, the point at which the difference between the two curves
will be a maximum.[3] The derived equation, in terms of the
number of responses to bar A, is $N_A = \dfrac{2 \log 2}{i}$ where i is the
parameter determining the rate of approach to the asymptote
of the growth function. Noting, as we shall see in a moment,
that Ramond's choice data showed that the block of trials on
which both the low and high drive groups exhibited a maximum
per cent choice of bar A had a midpoint which involved 36
reinforcements on bar A, we substituted this value for N_A in

3. The mathematical derivation of this equation is given in Appendix B.

the equation and solved for i. The obtained value of .0167 has been employed in the equation describing the growth curves of E shown in Figure 66. The value of the parameter is the same for both curves, for it should be recalled that the number of responses on bar B is half that on bar A.
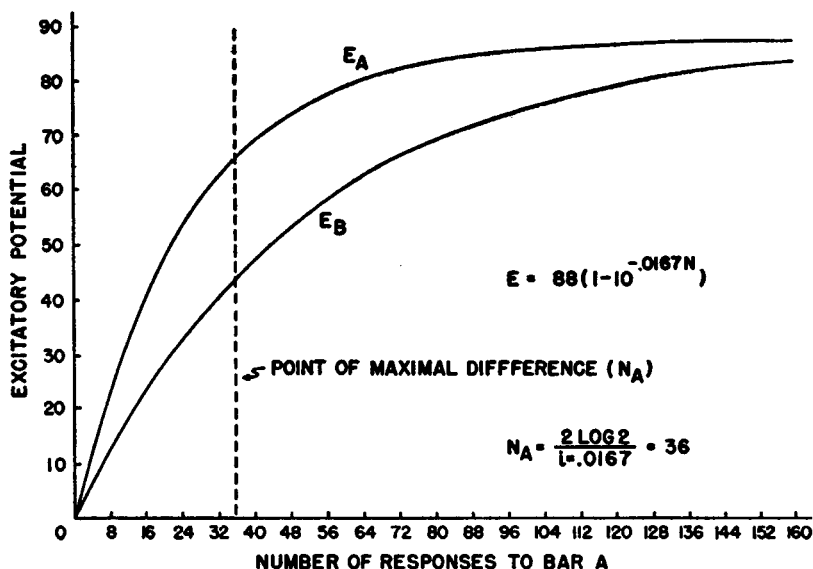
Fig. 66. Growth curves of reaction potential in two-response selective learning, where the ratio of occurrences of the competing reactions is maintained at 2:1 throughout training, plotted as a function of the number of occurrences of the more frequent response, R_A. As all other conditions are identical between the two responses, values of E_B are given by entering in the equation for E_A with $N = N/2$. The broken vertical line at trial 36 indicates the point at which $E_A - E_B$ is maximal as calculated from the equation for N_A shown in the lower portion of the figure.

Turning now to the choice data in Figure 67 it will be seen that the percent choice of bar A increased, as predicted, to a maximum and then decreased. Unfortunately, training was not sufficiently prolonged to establish the fact that both choice curves returned to chance. However, Ramond did report that the difference in per cent choice between the maximum per cent and the per cent on the last block of training trials for the two groups considered together was statistically significant.

Having determined the value of the parameter, i, in terms of the choice data it is interesting to ascertain how well it fits the speed data obtained on the forced trials to bar A. Figure 68 presents the speed data for the 22-hour drive group with an exponential curve based on the parameter value .0167 drawn
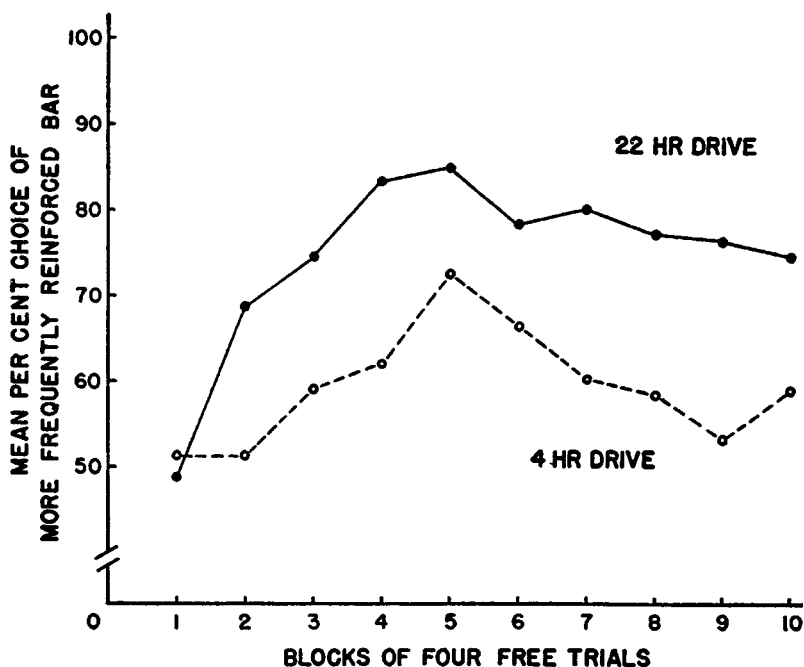
Fig. 67. Mean percentage choice of the more frequently reinforced bar (A) as a function of training in a double-bar apparatus (see Fig. 65) such that the ratio of responses at the two bars was maintained at 2:1. The two curves represent the data of different groups trained under 4 and 22 hours of food deprivation. (After Ramond, 1954b.)

through the empirical points. As may be seen the fit *beyond* the first four blocks of trials is extremely good. Recalling our analysis of the form of speed-of-response-evocation curves in Chapter 4 it will be remembered that the early portion of such curves does not accurately represent the growth of excitatory potential because of the presence of competing responses which depress or distort them downward. The later portion of

such curves is more or less free from such effects and thus more accurately reflects the growth of E.

Further evidence of this same type is provided in another study, a master's thesis by Stein (1953). Figure 69 shows the
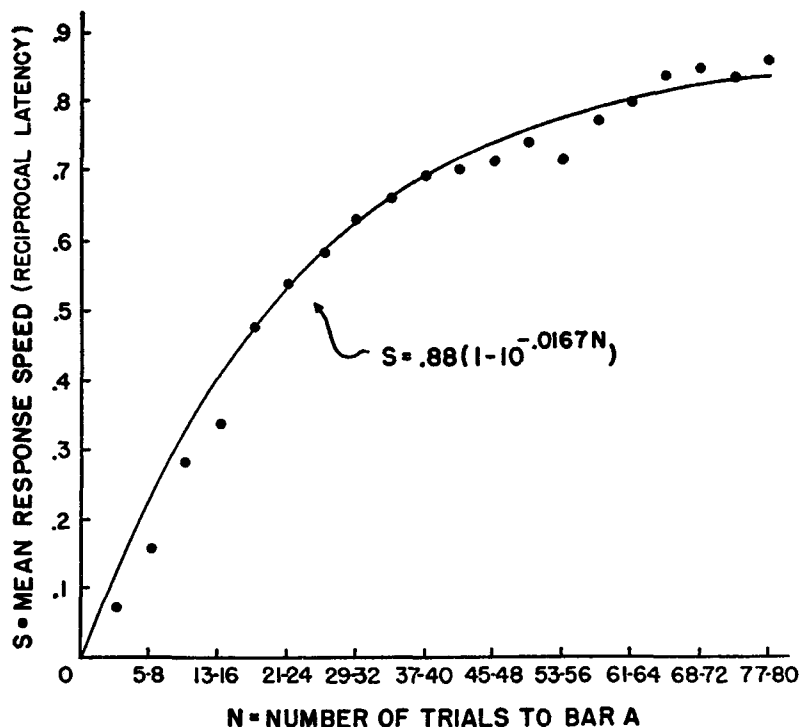
Fig. 68. Mean response speed on the instrumental (forced) trials to the more frequently reinforced bar in the 2:1 response-occurrence selective learning situation for the 22-hr. deprivation group (see Fig. 67). The exponential constant of the growth function fitted to these data was determined independently from the choice data by counting the number of trials to the more frequently reinforced bar following which maximal choice of that bar was obtained, entering this value into the equation for the point of maximal reaction potential difference (N_A), and solving for i. (After Ramond, 1954b.)

speed data obtained by Stein for bar A in the same experimental setup. However, the subjects used in this experiment were albino rats instead of hooded rats as used in Ramond's study. As may be seen, the value of the parameter determining

the rate of approach of the fitted exponential function to the speed data of these subjects was much smaller than in the case of Ramond's animals, being only .0118 as compared with .0167. According to this smaller value of i, the maximum per cent choice of the A bar should occur much later than in the
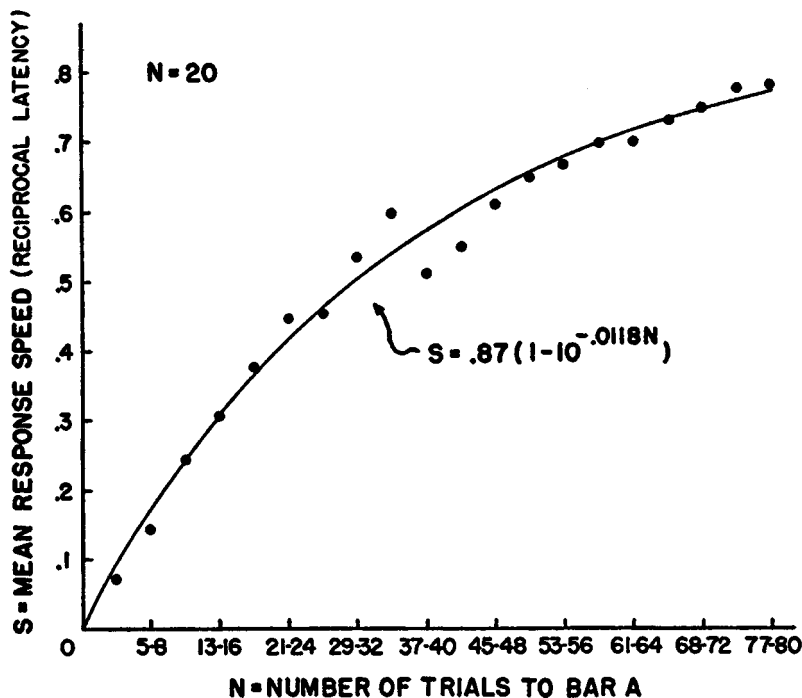
Fig. 69. Mean response speed on the instrumental (forced) trials to the more frequently reinforced bar in the 2:1 response-occurrence selective learning situation. The parameters of the smooth curve were obtained from the speed data by means of the conventional curve-fitting procedures. (After Stein, 1953.)

case of Ramond's study. The predicted trial would be 51.0. Figure 70, which presents the choice data for this experiment shows that the highest per cent choice was obtained in the block of trials the midpoint of which involved 52 trials to bar A. The capacity of the theory to interrelate these two sets of different data in this manner is, to say the least, quite encouraging.

If we now turn to the implications of our theory of the effect

of varying motivational level in this type of competitive response situation, the magnitude of the difference between the competing reaction potentials should be a positive function of the drive level. This is shown by the following algebraic derivation:

$$E_A = H_A \times D$$
$$E_B = H_B \times D$$
$$E_A - E_B = D(H_A - H_B)$$

What prediction will be made with regard to the variation of the per cent choice of the more reinforced bar (i.e., bar A)
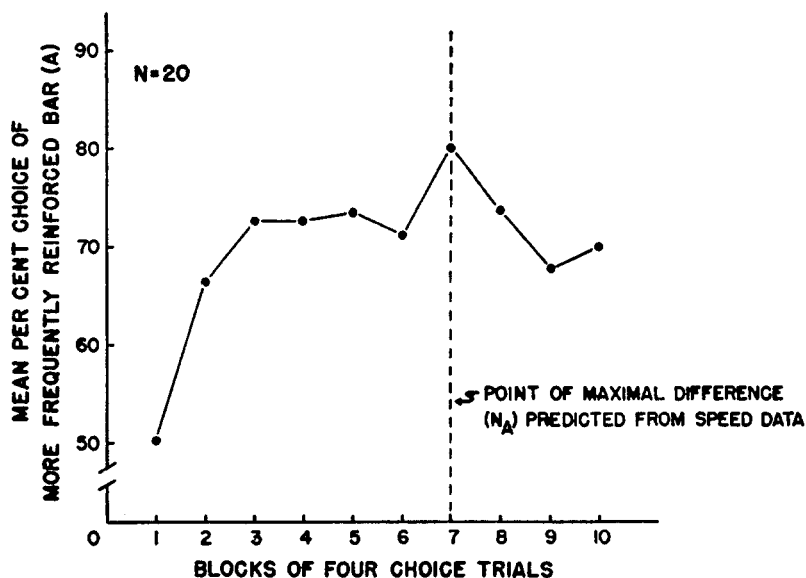
Fig. 70. Mean percentage choice of the more frequently reinforced bar on the free trials of the 2:1 response-occurrence selective learning situation. The broken vertical line at the seventh block of choice trials indicates the point of maximal choice predicted from the speed data shown in Fig. 69 by means of the equation for the point of maximal reaction potential difference. (After Stein, 1953.)

under different drive levels, however, will depend upon a number of other factors in addition to the magnitude of the difference between the competing E's. One of these that we have already mentioned is the absolute level of the competing excita-

tory potentials. Thus it will be recalled that the per cent choice of a response is a joint function of both the difference between the E's and their absolute levels.

Since the absolute level of the E's is also a function of the stage of training (i.e., habit strength) as well as the drive strength, it is apparent that the relationship is a very complex one that does not permit the simple prediction that the high drive group will always exhibit a greater per cent choice of response A than a group with a relatively lower drive. The prediction will depend upon both the stage of training and the absolute levels of drives of the two groups being compared.

We are at present engaged in working out the more complete function relating per cent choice of response A in this situation to the several determining variables. All that can be said at present is that if the combination of training and drive levels is such that the reaction potentials of both competing responses of the low drive group are fairly well above the threshold L then the higher drive group will exhibit a higher per cent choice of bar A than will the lower drive group. Since we are not as yet in a position to state whether this is the case in Ramond's particular experiment no prediction can be made as to the relative performance of his two drive groups. It is of some interest, perhaps, to note that Ramond's high drive group did choose bar A significantly more than did the low drive group.

Before going on to the next selective learning situation one further set of initial conditions needs to be considered, namely the initial preferences of the subjects for the two responses. There is considerable evidence to show that some animals exhibit strong initial preferences for one or other of the two alternative responses in such situations. On the possibility that such preferences represent differences in the initial habit strengths of the two responses it is necessary to control for this factor by employing only subjects that show either no initial preference or a slight preference in favor of response A. If this is not done the problem of prediction becomes even more complicated for, if such preferences do represent differences in habit strength, then differences in drive would have opposite effects on the strength of the preference for bar A, tending to increase the percentage in the case of subjects that prefer bar

A and to decrease it in the case of subjects that show an initial preference for bar B.

Situation Involving Differential Delay of Reinforcement. The second type of selective learning experiment that we shall discuss involves differential delay of reinforcement of the two responses. As conducted in our laboratory with the dual-bar apparatus, response to one bar (the short-delay bar) is followed by reinforcement in one second. In the case of the second, long-delay bar, the reinforcer follows contact with the bar after a longer period, e.g., five seconds. The training series is given in blocks of four trials, the first being a free-choice one in which both bars are illuminated. The last three trials are "forced" in such a manner that the number of trials to each bar is equalized.

The acquiring of a preference for the short-delay bar in this situation would be explained, according to the theory proposed in Chapter 5, in terms of either a differential K factor or a differential inhibitory factor (I) or possibly both. Whichever one or combination of these factors we assume, it may be shown that the difference between the excitatory strengths of the two responses would not be a function of variations of the drive level. The derivation of this implication in the case of the K factor is as follows:

$$E_\mathrm{s} = H \times (D + K_\mathrm{s})$$
$$E_L = H \times (D + K_L)$$
$$E_\mathrm{s} - E_L = H(K_\mathrm{s} - K_L)$$

Similarly, if we conceive the major effects of delay of reinforcement to be represented in the inhibitory factor (I), then, as shown by the following derivation, variations in drive will not, according to the theory, affect the difference between the competing reaction potentials:

$$\bar{E}_\mathrm{s} = H \times D - I_\mathrm{s}$$
$$\bar{E}_L = H \times D - I_L$$
$$\bar{E}_\mathrm{s} - \bar{E}_L = I_L - I_\mathrm{s}$$

Quite in contrast then to the first selective learning situation we considered, a high drive group would not be expected to exhibit a higher per cent choice of the short-delay bar than a low drive group at any absolute level of drive. Indeed, as may be shown by means of Figure 71, under certain conditions the

per cent choice of the stronger response will be inversely related to drive level. In this graph per cent choice of the stronger response to the short-delay bar is shown on the ordinate to the left. The abscissa indicates the absolute level of excitatory potential, and the values to the right represent the magnitude
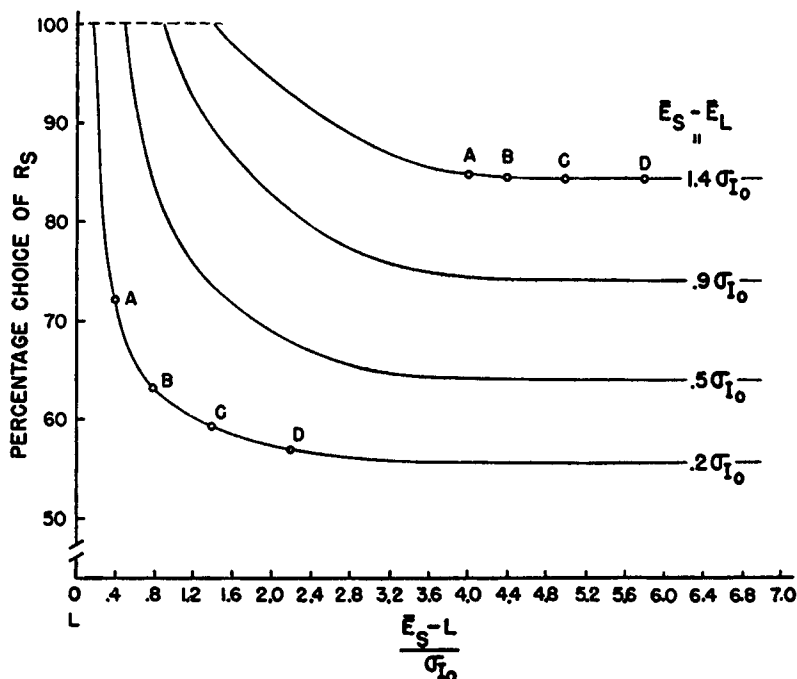
Fig. 71. Percentage choice of the dominant response (R_s) in two-response selective learning as a function of the absolute value of the reaction potential of R_s, with the differential reaction potential between the competing reactions as the parameter. The hollow circles indicate the expected choice performance of four different motivational groups at early (lower curve) and late (upper curve) stages of training. See text for a more complete interpretation of this diagram in terms of selective learning performance as a joint function of the stage of training and the motivational level, where choice is mediated by reinforcing the competing reactions after different delays.

of the difference between the competing reaction potentials (i.e., $\dot{E}_S - \dot{E}_L$). Now since $\dot{E}_S - \dot{E}_L$ is the same for groups run under different drive levels all such groups will fall at any stage of learning on the same curve. The lowest curve represents an early stage of learning, when the difference between

E_8 and E_L is relatively small. The higher curves represent successively later stages in learning with the differences $(E_8 - E_L)$ becoming larger and larger. Since the absolute level of E also varies with training trials the data of any group of subjects will not only move up to higher curves but will also move to the right. The hypothetical data of four differentially motivated groups of subjects (A, B, C, and D) at an early stage of learning are represented on the lowest curve of the graph. With training the position of these groups in the graph will move to the right (i.e., to a higher absolute level of E) and to higher curves (i.e., to greater $E_8 - E_L$ differences).

As may be seen the implications of the theory are that there will be an inverse relation between performance (per cent choice R_s) and drive level under conditions which keep the reaction potentials in the low range. These conditions may be specified as low initial habit strengths of the two competing responses, the early stages of selective learning and low ranges of drive level. Thus it would be expected that a differential in favor of lower drive groups would tend to be present in the early stages of training and at low absolute levels of drive. In the high range of drive level there should be no difference between groups differing in drive strength. Another prediction that may be made is that if a great amount of preliminary training is given on both bars separately prior to presenting them together no difference will be found in the choice performance levels of the different drive groups. In this instance the four groups would be well out to the right on the lowest curve at the beginning of the choice trials.

Unfortunately we do not as yet have any data with which to test these predictions except a single doctoral investigation that was conducted a few years ago by Loess (1952). Loess compared two groups of rats under three and 22 hours' hunger drive in the dual-bar situation, response to one bar having a reinforcement delay of one second and response to the other a delay of five seconds. The results of this study were somewhat equivocal. While the high drive group performed slightly better on the choice trials than the low drive group, the difference was not significant. We are at present engaged in a more extensive investigation involving a number of groups at different drive levels which we hope will provide a more definitive test of

the theory. It is of some interest to note here that the results of Loess' experiment differed from those obtained in the first type of selective learning problem we discussed. In the latter situation, it will be recalled, higher drive did lead to a significantly higher per cent choice of the stronger response.

Before we leave this case of selective learning, attention should be called to the point that the number of responses to the two bars was equalized. This implies, theoretically, that the habit strengths of the two competing responses were equal. If, on the other hand, we permit free choice the number of responses to each bar will not be equalized, for with the differential development of K or I, the excitatory potential of the short-delay response will become stronger than that of the long-delay response and a greater and greater proportion of the responses will come to be made to the short-delay bar. In this circumstance a difference in the habit strengths of the two responses will develop, and this would mean that differences in the excitatory strengths of the two responses would vary with the level of drive. It is thus apparent that quite different predictions would be made as to the effects of variation of drive in this type of situation depending upon whether or not forced trials are employed to equalize the number of responses to each bar or whether free-choice trials are permitted.

Situation Involving Reinforcement and Nonreinforcement. The final example of simple selective learning that we shall consider is the more or less standard situation in which one of the alternative responses is reinforced and the other receives no reinforcement, with no correction being permitted. Providing the number of occurrences of the two responses is kept equal by some kind of forcing technique, variation of drive, as in the previous case, would not lead to any difference in the strengths of the competing reaction potentials. The derivation of this implication is as follows:

$$E_+ = H \times (D + K)$$
$$\bar{E}_- = H \times D - I$$
$$E_+ - \bar{E}_- = KH + I$$

Here, again, it will be seen that the difference between the competing E's is not a function of D. On the other hand, if all of

the trials are free the subjects will come to make a greater and greater proportion of positive responses with the result that the habit strengths will become different. Under this condition drive variation would make a difference, the higher the drive the greater the difference between the competing reaction potentials. Thus we may predict that, whereas the equated-response situation would never be expected to lead to higher performance with higher drive, the free-response situation would.

Considering further the theoretical implications of the equated-response case of this kind of selective learning, it should be noted that the same predictions may be made concerning it that were made in the differential-delay situation. That is, in the early stages of learning, particularly at very low levels of drive, there should be an inverse relation between degree of motivation and choice performance. At higher absolute drive levels this relationship would tend to disappear, differences in drive strengths having little or no effect on performance.

TABLE V

Mean number of trials required to reach learning criterion as a function of food deprivation in selective learning under conditions of equated frequency of occurrence of the competing reactions. (From data published by Teel, 1952.)

| | Trials to Learning Criterion | |
Group	Mean	SD
1	15.33	4.87
7	16.19	5.65
15	17.24	4.72
22	17.62	4.56

Again, evidence bearing on these theoretical implications is woefully lacking. Nevertheless there are a few studies that have some relevant data. For example, Teel and Webb (1951) found that rats trained in a single-choice T maze in which responses to each side were equalized performed at about the same level under 23 hours' food deprivation as when satiated. More interesting are the findings of another part of the same experiment reported by Teel (1952). Shown in Table V are the number of trials required to reach the learning criterion by four groups of rats deprived of food for 1, 7, 15, and 22 hours respectively.

As may be seen the results are highly suggestive of an inverse relation between speed of learning and strength of the hunger drive.

On the other hand, Tolman and Gleitman (1949) found, just as would be expected from our theory, that a group of rats motivated by 46 hours' food deprivation performed at a higher level in a *free-responding situation* than a group that had been deprived for 12 hours. There are a number of aspects of this study, however, that make it a questionable test of our theory. Thus the correction rather than the noncorrection method was used, and the learning involved place and response learning rather than the standard type of T-maze learning. While time will not permit an analysis of these variations of selective learning, as far as my conception of them is concerned the same predictions with respect to drive variation would be expected as in the standard T-maze situation. The effects of the correction method are less easy to evaluate.

In concluding this discussion of some of the implications of our theory for these simple selective learning situations I should like to return to a point that was raised earlier. I refer to the importance of taking into account the initial and boundary conditions as well as the laws and hypotheses of a theory when applying it to particular experimental situations. The different predictions concerning the effects of varying drive level in these several situations provide an excellent object lesson in this regard. Perhaps also this discussion will help to explain my puzzlement concerning statements in the psychological literature such as that failure to find a performance difference with variation of drive level in selective learning situations not only disproves a theory that predicts performance is related to drive level in classical and instrumental conditioning but also shows the futility of attempting to explain more complex behavior in terms of a theory developed from simpler phenomena (Harlow, 1953). The most charitable interpretation that can be placed on this sort of thing, the occurrence of which is discouragingly frequent in our literature, is that many psychologists do not fully realize the low level of abstraction of their theories and the importance of the boundary conditions in making logical deductions from them.

ANXIETY (DRIVE) LEVEL AND PERFORMANCE IN HUMAN LEARNING

The second area of experimentation that we shall employ as a means of demonstrating the possibility of extending our theory to more complex behavior involves human verbal learning. The experimental data were obtained in a series of studies conducted in our laboratory in which the Taylor anxiety scale was employed as a means of varying drive level in human subjects. These studies are closely related to those we have just been considering in that they also are primarily concerned with the effects of different drive levels on performance.

But first let me clarify the logic of these experiments for, if one may judge from the literature, there is much misunderstanding. For example, one criticism made of them is that since we do not have independent evidence that the test *really* measures emotionality we cannot legitimately assume that differences on the test reflect differences in the general drive level, D, of the subjects. This is comparable, of course, to saying that since we do not have direct evidence that a strong puff produces more emotionality than a weak puff we are not justified in assuming that differences in puff strength produce differences in level of D. Both statements, of course, are untrue, for as a matter of fact we can and do make such assumptions. Indeed this assumption or hypothesis is one part of the total set of assumptions, i.e., the theory, that we are testing. We test this particular assumption and other relevant parts of the theory by deriving implications concerning differences to be expected in conditioning and other types of learning situations. Confirmation of these deductions lends support to the theory, including the hypothesis about the relation of the scale scores to level of D. Obviously they don't prove the theory, for no theory is ever proved in science.

Another misconception that appears over and over again in the discussions of these studies is that our theory is disproved because an alternative interpretation can be put forward to account for some of the findings. Ignoring the point that these interpretations are typically ad hoc with respect to one particular finding and are usually not related to the findings from

other studies, it should be clear that such alternative hypotheses are just rival hypotheses, nothing more. They do not in any way disprove our hypothesis as has sometimes been implied.

Early Studies Involving Competing Responses. Turning now to the studies themselves it will be recalled from the last chapter that a number of experiments involving classical defense conditioning have supported the theory, subjects from the high end of the scale (i.e., high D subjects) exhibiting a higher level of response than those from the low end (i.e., low D subjects). In more complex learning situations involving competing responses, however, the effect of drive-level variation will depend, as we have seen, on a number of different factors to which careful consideration must be given. Particularly important are the presence and the direction of the differences in the habit strengths of the competing responses. If the experimental situation does not develop differences in the habit strengths then drive variation will not affect performance. If there is a difference in habit strength in favor of the correct response over the incorrect response or responses, then the higher the drive the better the performance.[4] If, on the other hand, the correct response has a lower habit strength than the competing response or responses then high drive will be detrimental to performance, at least in the early stages of learning before the direction of the habit differences is reversed.

The first studies that we conducted involving more complex learning problems employed serial verbal and maze learning. Making use of the known fact that anticipatory and perseverative tendencies in serial learning produce strong competing response tendencies it was predicted that high-anxious subjects would perform more poorly, i.e., make more errors on this type of problem, than low-anxious subjects. Three separate experiments, one involving a verbal maze (Taylor and Spence, 1952), one a stylus maze (Farber and Spence, 1953), and one rote serial learning (Montague, 1953), provided evidence support-

4. Increasing drive under this condition obviously increases the magnitude of the differences between the competing responses; not considered here is the possibility that under an increased drive more responses will become superthreshold and thus likely, because of oscillation, to provide more competing responses. Ultimately this latter matter will need to be taken into consideration.

ing the theory. In all three instances the high-anxious subjects were inferior to the low-anxious subjects.

Now it is important to note that in instituting these studies our primary, indeed our sole, interest in the anxiety scale was as a possible index to drive level. We were not unaware, however, that there are other possible differences in persons who score at extremes of this scale than a difference in level of drive. In particular, we were sensitive to the possibility that the differences in anxiety were also accompanied by differences in response tendencies, e.g., degree of susceptibility to distraction, that might interfere with the performance of the learning task. Thus we were well aware of the early study of Estes and Skinner (1941) that had shown that fear could depress the rate of instrumental responding. Furthermore, the series of studies of Amsel (1950a, 1950b) and his colleagues (Amsel and Maltzman, 1950; Amsel and Cole, 1953) which has most clearly distinguished between the facilitating and depressing effects of emotionality and the conditions determining them was actually started in our laboratory. To claim, as one critic has, that we completely overlooked this aspect of emotionality is simply not true (Child, 1954). As a matter of fact we deliberately chose or attempted to arrange conditions in which such interfering aspects would be minimal. For example in the verbal-maze situation a sufficiently long anticipatory interval was provided to permit the subject always to make one or other of the choices.

Nevertheless, it is true that one cannot be certain in these serial learning situations that the decrement in performance was not, in part at least, due to greater task-interfering responses on the part of the more anxious subjects. These situations simply do not permit separating out the role played by this factor and that of the drive level per se. For this reason and also because one has little or no knowledge in these situations of the relative strengths of the specific correct and incorrect S-R tendencies, we were interested in designing learning situations in which it would be possible to manipulate in some known manner the relative strengths of the competing response tendencies. The first of this series of studies has already been published by Ramond (1953), who found that under the condition in which the weaker of two responses was correct the high-

anxious subjects, as predicted by the theory, did significantly poorer than the low-anxious subjects. Under the condition in which the stronger of the two responses was correct the high-anxious subjects responded somewhat better than the low-anxious subjects initially, but the difference disappeared during the course of learning and even reversed itself, although not significantly. Ramond offered a possible explanation of these latter findings in terms of the effects of stimulus generalization. Whether or not his interpretation is correct is not so important as is the fact that in contrast to the first condition there was no difference between high- and low-anxious subjects. Since the effects of task-interfering responses would be equal for the two kinds of learning items which were intermixed with each other in the same list, the inferior performance of the anxious subjects in the one situation must be accounted for by some other factor. Our interpretation was that it is due to the fact that the greater drive level of the high-anxious subjects increased the unfavorable difference in the competing excitatory potentials in the direction of the incorrect response and thus led to a greater probability of occurrence of such erroneous responses.

Studies Employing Paired-Associates Learning. With these experimental results as background we next set out to design a learning problem in which we could arrange for each of the correct responses to be initially much stronger than its particular competing response and which, furthermore, would permit the reversal of this condition. The situation selected was paired-associates learning. In this type of learning the subject must learn to respond to the presentation of a stimulus word by anticipating a paired response word. By employing different orders of the paired words the development of remote associations, so prominent in serial learning, is minimized.

Paired-associates learning, as shown in Figure 72, may be conceived as consisting of a set or series of *more* or *less* isolated stimulus-response tendencies that become strengthened as training proceeds. Theoretically, if these stimulus-response items were entirely isolated from one another so that associative tendencies existed only between each stimulus word and its own paired word, then essentially we would have a situation similar

to that of classical conditioning. The main difference is that whereas conditioning involves the strengthening of a single S-R tendency this situation involves the strengthening of a number of different S-R tendencies simultaneously. While it may not be possible to obtain complete isolation of the individual S-R items, we do know how, on the basis of existing

$$S_1 \longrightarrow R_A$$
$$S_2 \longrightarrow R_B$$
$$S_3 \longrightarrow R_C$$
$$\vdots$$
$$S_{12} \longrightarrow R_L$$
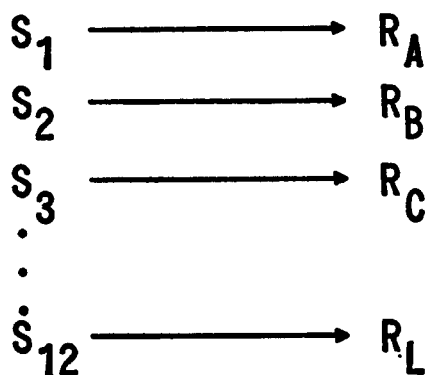
PAIRED ASSOCIATE LEARNING

Fig. 72. Diagrammatic representation of paired-associates learning. The R's and S's joined by arrows represent a set of stimulus-response tendencies that become strengthened as training proceeds. Taken separately, the strengthening of each stimulus-response association may be viewed as simple classical conditioning. However, as several S-R tendencies are being strengthened simultaneously such theoretical isolation of the individual stimulus-response items may be approached experimentally only under special conditions.

experimental knowledge, to approach this limiting condition with its minimal competition among S-R's. Similarly we know how to vary conditions so as to increase the amount of competition among them.

One of the most important factors determining the degree of isolation of the paired S-R's is the degree of generalization that occurs between them. This, in turn, is a function of the

synonymity and/or formal similarity among the stimulus and response items. With minimization of this factor, there will be little or no generalized tendency for S_1 to elicit other responses than R_A, S_2 to elicit responses other than R_B, and so on.

A second important factor in such paired-associates learning is the existing strength of the associative connections between each stimulus word and its paired response word. As the result of past experience words tend to become associated with other words in varying degrees, and for each word the hierarchy of associative strengths tends to be similar for individuals in the same cultural group. Such differences in the strengths of associative connections between words in a language are exemplified by the word association lists of Kent and Rosanoff (1910).

It is readily apparent that we may also employ this factor of initial associative strength to control the extent to which each stimulus word will tend to elicit only its own paired word and also the degree to which it will tend to elicit other response words than the one with which it is paired. Thus we can arrange to pair each stimulus word with a response word with which, as the result of past verbal experiences, it is highly associated and at the same time make sure that the associative connections between each stimulus word and each of the other response words are low or nonexistent. Such a condition would obviously help to minimize the likelihood of there being for a stimulus-response pair a competing response tendency of any degree of strength. Thus a list of paired associates in which the paired words had high initial associative connections and in which the degree of synonymity of the stimulus and response words was minimal would provide a noncompetitive learning situation in which, theoretically, high-drive (i.e., high-anxious) subjects should perform at a higher level than low-drive (low-anxious) subjects. Contrariwise, we may construct a paired-associates list with a high amount of competition in which the opposite finding should occur; that is, the high-anxious subjects should perform more poorly than the low-anxious. There are a number of different ways in which such competition may be introduced, one of which will be described after we have presented the results with the noncompetitive type of list.

Figure 73 presents the results of an experiment that were

obtained with a list of words designed to minimize competition.[5] Thus synonymity among both stimulus and response words was minimal, and the initial strengths of the associative con-
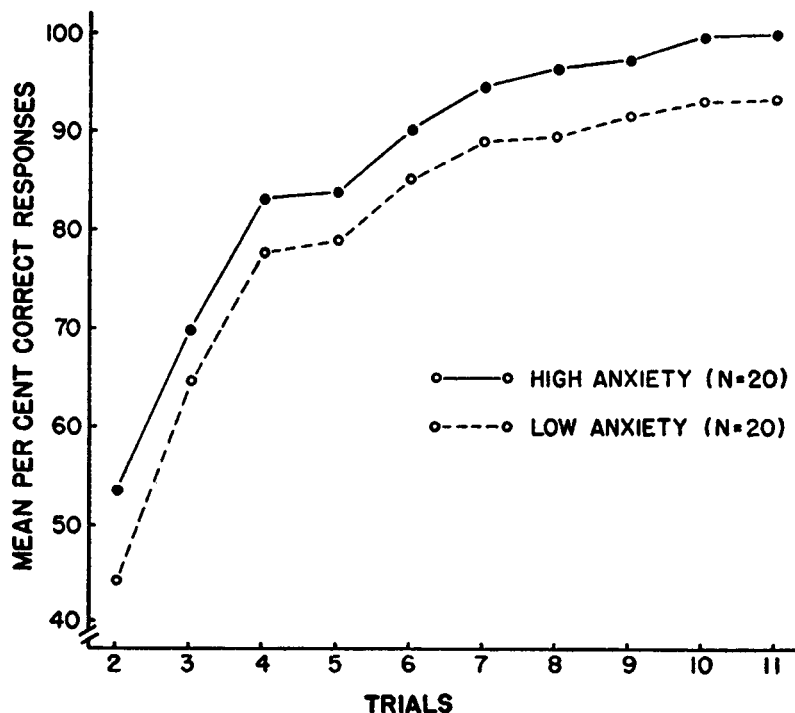
Fig. 73. Paired-associates learning as a function of anxiety under conditions of minimal inter-word pair competition and high initial stimulus-response associative strength. In line with prediction, the performance of the high anxiety group may be observed to exceed that of the low anxiety group throughout training.

nections, ascertained from Haagen's word list (1949) which provides scaled values of closeness of association between pairs of words, were high. The standard anticipation method was

5. This study and the remaining ones involving paired-associates learning to be described in this chapter were conducted as part of a project carried out under contract with the Office of Naval Research. Assisting in the conduct of these experiments were Rhoda Ketchel, Donald Haggard, Howard McFann, and John Taylor.

employed, and the two groups of subjects were from the upper and lower 20 per cent of the distribution of anxiety scale scores. As may be seen the high-anxious subjects performed at a higher level from the first anticipation trial. A statistical analysis of the over-all number of errors and trials to learn revealed both measures to be significant beyond the .05 level of confidence.
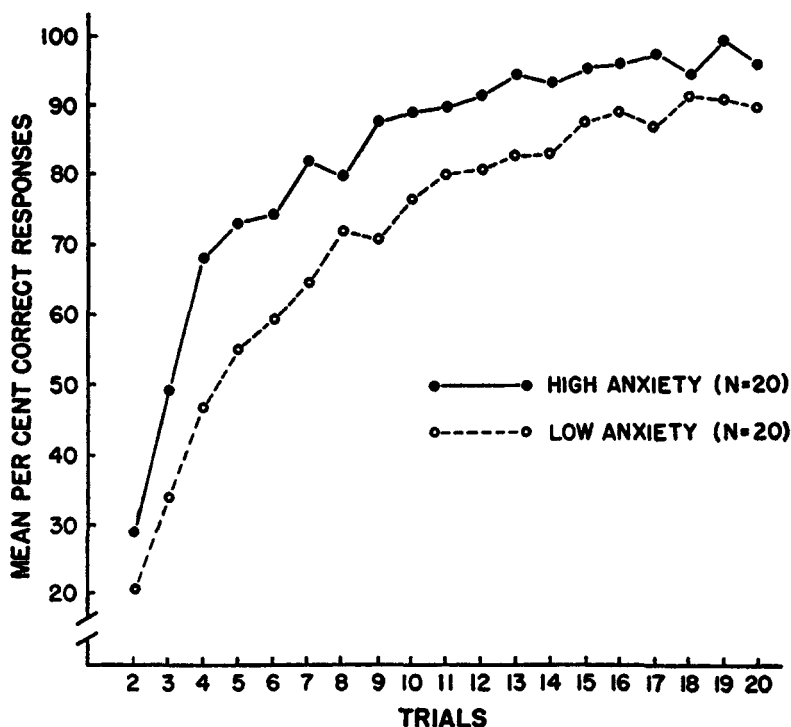
Fig. 74. Paired-associates learning as a function of anxiety under conditions of minimal inter-word pair competition and low initial stimulus-response associative strengths Again, the expectation of superior performance on the part of the high anxiety group may be seen to have been substantiated.

Figure 74 presents the data of a second experiment in which the average initial associative strength between the paired words was somewhat lower than in the first experiment. Consistent with this, both groups began at a considerably lower level of performance and required more trials to learn. Again, it will be seen that the high-anxious group did better than the low-

anxious subjects. In this instance the error measure was significant at better than the .02 level. Since all of the subjects did not meet the learning criterion in 20 trials no trials-to-learn measure was available.

The results of a third experiment, just completed, are shown in Figure 75. In this study 16 paired adjectives were
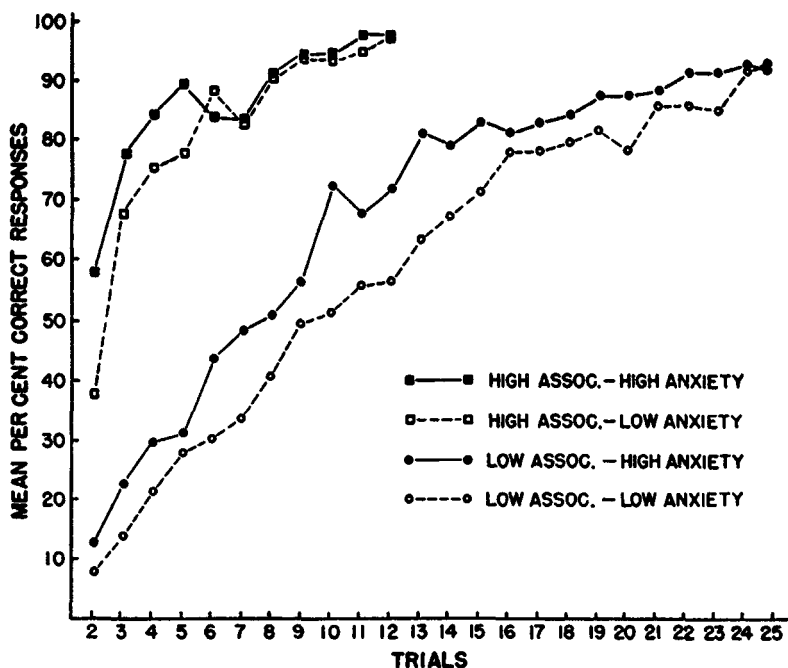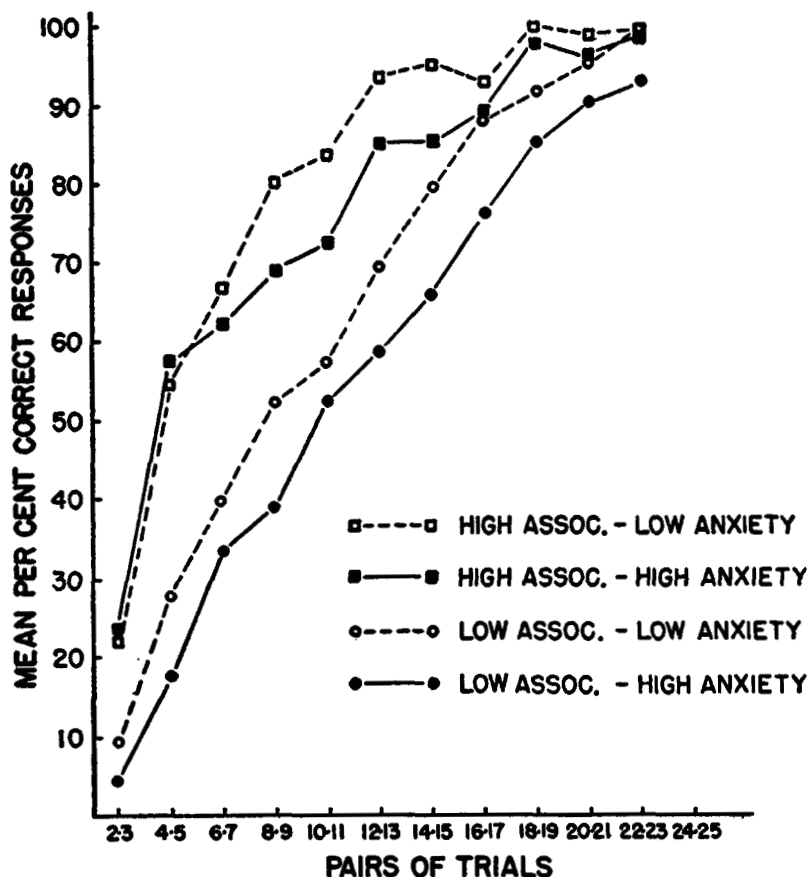

Fig. 75. Paired-associates learning as a function of anxiety under conditions of minimal inter-word pair competition. Word pairs of both high and low initial associative strengths were interspersed in the same training list but were analyzed separately. The upper pair of curves represents the data obtained with the high-association word pairs, the lower that obtained with the low-association word pairs. As expected, the high anxiety group may be observed to have performed at a superior level in both cases.

employed, eight in which the associative connections between the paired words were low and eight in which they were high. Separate curves have been drawn for these two sets of paired words. As would be predicted by the theory there is a difference in performance from the beginning for the highly associated

pairs, whereas the difference is small to begin with for the low associated pairs but gets larger as their habit strengths increase. Unfortunately, these results, which were obtained with women subjects, were not reproduced in the case of men. The latter showed only a slight but quite insignificant difference in favor of the high-anxious subjects. The differences in the case of the women were significant at better than the .01 level for the low associated pairs and at slightly short of the .05 level in the case of the high associated pairs.

The evidence of these three experiments clearly indicates that, in spite of any possible greater tendency for high anxiety to be accompanied by more task-interfering responses, the highly anxious subjects performed in a superior manner to the low-anxious subjects. This finding thus refutes the interpretation that anxiety acts as a drive or energizer only for defense responses, a position that is also untenable in the light of the findings with animals that the perseverative effects of shock facilitate eating and drinking behavior. However, that so-called task-interfering responses may play an important role in this particular kind of paired-associates learning is, perhaps, shown by the results of still another study that employed the same list of 16 paired associates as was used in the last described experiment. In this study the anticipation interval was only 1.1 seconds instead of 2.2 seconds as in the previous experiment. Under this condition, in which the learning problem was very difficult and quite stressful, there was no difference whatever between anxious and nonanxious subjects in the case of either men or women. It is possible that under this condition a greater amount of task-interfering behavior on the part of the anxious subjects offsets the greater energizing role of their higher drive level. But whatever the correct interpretation may be we see here another example of how the results of such experiments depend upon other conditions than the main variable being investigated.

The final set of experiments that I shall present involved the learning of paired associates in which there were stronger, specific competing responses present. In the first of these studies a list of 12 paired adjectives was used. In the case of four of these pairs the associative connections were very strong. The remaining eight pairs, made up in the following manner, in-

volved no or weak associative connections. For each of the four stimulus words paired with closely associated words there were two synonymous stimulus words that were paired with words with which they had no associative connection. Thus, as shown in Figure 76, in the case of each triad of synonymous stimulus words, two were paired with response words with which they were weakly associated, if at all, and one with a highly associated word. The stimulus words, S_1' and S_1'', being highly synonymous with S_1, also had high initial associative connec-
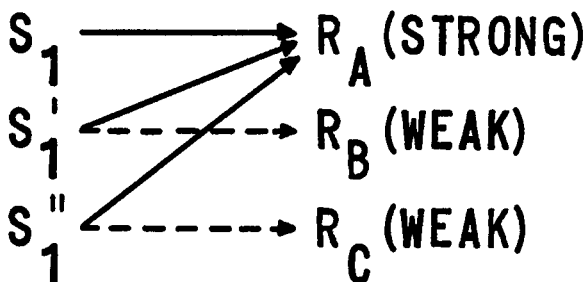
Fig. 76. Diagram showing how inter-word pair competition may be manipulated experimentally. $S_1 \longrightarrow R_A$ is a stimulus-response tendency with high initial associative strength; $S_1' \longrightarrow R_B$ and $S_1'' \longrightarrow R_C$ are stimulus-response tendencies with low initial associative strengths. Further, S_1, S_1', and S_1'' are synonymous, so that strong associative tendencies exist also between R_A and S_1' and S_1'' (represented by unbroken arrow shafts).

tions with R_A. As a consequence the learning of the word pairs involving these stimuli, i.e., $S_1' \longrightarrow R_B$ and $S_1'' \longrightarrow R_C$, involved a strong competing response, R_A, one in fact that was stronger than its paired response. In the case of these particular word pairs then, we should expect the anxious subjects to perform more poorly than the nonanxious subjects.

The implications of the theory with respect to the relative performance of high- and low-drive subjects on the four stimulus-response pairs that have high original associative connections (e.g., $S_1 \longrightarrow R_A$) are more involved. At the very beginning of learning, of course, the performance of the high-drive subjects should be superior to that of the low-drive group.

If properly chosen these stimulus words should have little if any initial associative tendencies to R_B or R_C. However, once the subjects begin to learn the other pairs, (e.g., to learn
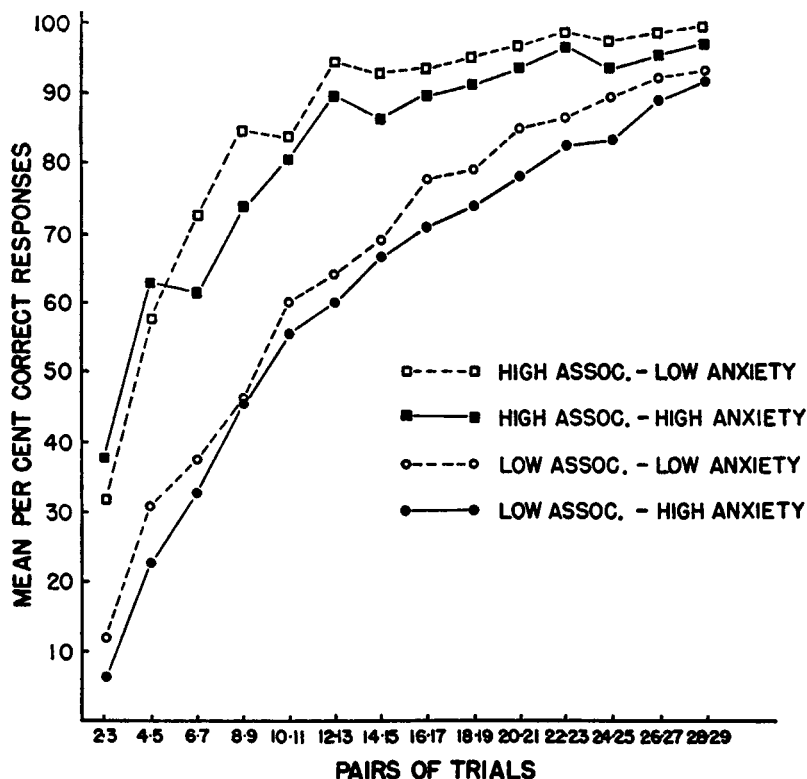

Fig. 77. Paired-associates learning as a function of anxiety under conditions of high inter-word pair competition. Word pairs of both high and low association value were interspersed within the same training list but were analyzed separately. In line with prediction, the performance of the low anxiety may be seen to have been superior throughout training on the low-association word pairs and initially inferior but subsequently superior later in training on the high-association word pairs.

$S_1' \longrightarrow R_B$ and $S_1'' \longrightarrow R_C$) there will develop a generalized habit for S_1 to evoke R_B and R_C (principle of generalization of associative or habit strength). Since the excitatory potential

from S_1 to these responses (R_B and R_C) should reach superthreshold values sooner for the high-drive group than for the low-drive group we should expect these responses to intrude and block the correct response (R_A) earlier (and more frequently) in the case of the high-drive group. Thus we would be led to predict that the initial superiority of the high-drive group should tend to disappear during training.

While the results of the experiment were in the predicted direction, they were not statistically significant. Thus, as Figure 77 shows, the highly anxious subjects were inferior to the non-anxious subjects on the eight nonassociated pairs that had strong competing responses. In contrast the anxious subjects started out at a higher level than the nonanxious subjects on the four pairs in which the paired response was initially highly associated. Despite the lack of statistical significance the pattern of the results was almost an exact duplicate of the findings obtained by Ramond in his similarly designed experiment involving choice between two alternative verbal responses. Furthermore, as Figure 78 reveals, a repetition of the paired-associates experiment in which two of the stimulus words had only one synonymous stimulus word each and two had two synonymous stimulus words as in the previous experiment again produced the same pattern of data. The remarkable similarity of the findings in all three experiments suggests that they do not represent a chance phenomenon even though the usual statistical criteria were not met.

Considering the results of these anxiety scale experiments as a whole we are still inclined to stick by our original hypothesis that differences in this scale act like differences in drive in that they produce the kinds of results that drive differences would, theoretically, be expected to produce. This, it should be noticed, does not deny the importance under certain conditions of other possible effects of anxiety differences, such as, for example, the differential extent to which task-interfering responses, distraction and so forth, occur. I suspect that, as yet, we have hardly begun to uncover all of the complexities that underlie even such simple learning behavior as is involved in these situations. I also believe that progress toward our common goal of a more complete understanding of these and other behavior phenomena will be greatly facilitated if psychologists will but

learn to make more effective use of theories instead of taking the
negativistic attitude that theories are something to be proved
wrong by fair means or foul. With few exceptions one can pick
up almost any volume of our journals and find innumerable
instances in which the writer in his headlong rush to demolish

Fig. 78. Paired-associates learning as a function of anxiety under conditions of
high inter-word pair competition. See Fig. 77 and text for further explanation.

some theory never bothered either to ascertain whether the
conditions specified by the theory were met or, in the cases in
which this was not possible, to consider carefully what the joint
implications of the new conditions and the theory might be.
Also a more positive approach, such as attempting to see what
modifications of the theory are necessary to make it work where

it fails or, if this is not possible, to suggest a new theoretical formulation, is more likely to lead to new discoveries and eventually to a better and more comprehensive understanding of the phenomena.

SUMMARY AND A FINAL WORD

In concluding this work I should like to state once more its aim. Beyond the first purely historical chapter the primary concern has been with a very circumscribed area of experimental study, namely, simple learning phenomena. Within this realm an attempt has been made to indicate the nature of some of the experimental variables studied and the empirical relations (laws) that have been discovered. The limitations of time precluded a complete treatment of even those areas considered and did not permit any discussion of such important problems as experimental extinction, partial reinforcement, stimulus dynamism, etc. However, our primary concern was not that of detailing all of the available empirical knowledge but rather one of attempting to exhibit by way of example the problems involved in providing for the integration by means of theoretical concepts of more specific bits of knowledge. This involved, first, the formulation of a theoretical structure involving a system of concepts and postulated interrelations which, in combination with the different initial and boundary conditions peculiar to each experimental situation, would provide for the derivation of the many specific laws obtaining in simple conditioning studies. A second task was that of extending the theory to more complex types of learning situations involving competition among responses.

Finally, I should like to return briefly to a matter that was discussed in the first chapter—namely the criterion of what is significant research in science. Insofar as any realm of observable data presents a consistent set of regularities, it represents a genuine set of phenomena that may legitimately be the object of scientific curiosity and endeavor. The question of whether any particular realm of behavior phenomena is more real or closer to real life and hence should be given priority in investigation does not, or at least should not, arise for the

psychologist *as scientist*. If there is any criterion at all beyond that of the personal curiosity of the individual as to what is important, it is that areas of behavior that lend themselves to the degrees of control and analysis necessary for the formulation of abstract laws and comprehensive theories should be given priority. To the person whose primary interest is that of building a scientific body of knowledge about behavior this is no problem. The laws of behavior we have been studying suggest that such a person will tend to select such areas automatically by virtue of the greater successes and rewards they provide. It is to be hoped that more and more psychologists will come to be dominated by such purely scientific interests.

Method of Computing Per Cent Choice of One Response over a Second in a Choice Situation

THE PROBLEM is to determine the expected probabilities of occurrence of two mutually incompatible responses, R_1 and R_2, as their momentary reaction potential ($\dot{E}$) distributions move, with the difference in excitatory potential (d) constant, across the reaction threshold (L), for any selected value of d. In lieu of a straightforward mathematical solution to this problem, an approximation involving the construction of tables has been employed. These tables can be refined to any arbitrary level of precision, and reasonably accurate ones do not demand unduly laborious calculations.

The problem reduces to an elemental form: the calculation of the probability of R_1 happening when the $\dot{E}$ distributions are in one given position relative to the threshold for one given excitatory potential difference. Once achieved, it is a relatively simple matter to extend the solution over a range of selected positions of the $\dot{E}$ distributions for the given value of d and then over these sets of positions for various values of d. Figure 79 depicts the elemental case in graphic form. The upended bell-shaped areas are the $\dot{E}_1$ and $\dot{E}_2$ distributions assumed to be normal in form and truncated $\pm 2.5\sigma$ from the mean ($M_{\dot{E}}$), with standard deviation, $\sigma_{\dot{E}} = \sigma_{I_0} = 1$. Also shown graphically are the definitions, $d = E_1 - E_2$, and $M_{\dot{E}} = E - 2.5\, \sigma_{I_0}$. From these definitions it follows that

$$(1) \quad d = (M_{\dot{E}_1} + 2.5\, \sigma_{I_0}) - (M_{\dot{E}_2} + 2.5\, \sigma_{I_0})$$
$$= M_{\dot{E}_1} - M_{\dot{E}_2},$$

also depicted in Figure 79. Finally, $\dot{E}_{L_1}$ = superthreshold $\dot{E}_1$, and $e_1 = (\dot{E}_1 - M_{\dot{E}_1}) \div \sigma_{I_0}$, or $\dot{E}_1$ in standard score form.

Generally, the solution for the elemental case involves the determination of the total number of expected occurrences of

both R_1 and R_2 (ΣR_1 and ΣR_2); the probability or percentage of times that R_1 will happen ($R_1\%$) is then given by:

$$(2) \quad R_1\% = \frac{100 \times \Sigma R_1}{\Sigma R_1 + \Sigma R_2}$$

Three steps are involved in the calculation of ΣR_1. First, the probability of R_1 happening given the occurrence of any $\dot{E}_1$ must be ascertained. Two conditions must hold if an R_1 is to occur; (a) $\dot{E}_1$ must exceed $\dot{E}_2$, and (b) $\dot{E}_1$ must exceed L. Condition b will be satisfied if we consider only superthreshold $\dot{E}_1$'s ($\dot{E}_{L_1}$). The probability of an R_1 happening given the occurrence of $\dot{E}_{L_1}$ ($P\{R_1 \mid \dot{E}_{L1}\}$) is then the same as the probability that $\dot{E}_{L_1}$ will exceed $\dot{E}_2$. From the preceding assumptions and definitions and on the assumption that a trial is terminated only following the occurrence of either R_1 or R_2 we may write

$$(3) \quad P\{R_1 \mid \dot{E}_{L_1}\} = f\left[\frac{\dot{E}_{L_1} - M_{\dot{E}_2}}{\sigma I_o}\right]$$
$$= f\left[\frac{\dot{E}_{L_1} - (M_{\dot{E}_1} - d)}{1}\right]$$
$$= f(e_1 + d)$$

where $f =$ normal probability integral. The reasoning represented by equation (3) is explicated graphically in Figure 1. The probability of R_1 given the occurrence of $\dot{E}_{L_1}$ is the relative frequency of those $\dot{E}_2$'s which are smaller than $\dot{E}_{L_1}$. This probability value is shown as the shaded area of the $\dot{E}_2$ distribution. As indicated in equation (3), this area is proportional to the area under the unit normal curve from $-\infty$ to $e_1 + d$.[1]

Having calculated $P\{R_1 \mid \dot{E}_{L_1}\}$ we proceed to the next step in the determination of ΣR_1 by computing the *total number of times* that R_1 can happen for the given value of $\dot{E}_{L_1}$. This value is the probability of R_1 given $\dot{E}_{L_1}$ times the frequency of $\dot{E}_{L_1}$. This frequency is the ordinate of the $\dot{E}_1$ distribution at $\dot{E}_{L_1}$

---

1. It might be expected on the basis of the truncation assumption that the integration should be taken from -2.5 to $e_1 + d$. Since negligible errors of approximation are involved in both cases when tables of the normal probability integral are used for the calculation of the area values, the more convenient value, $-\infty$, has been used.

which is proportional to the ordinate of the unit normal curve at $x/\sigma = e_1$. Finally, we sum the product, $P\{R_1 \mid \dot{E}_{L_1}\}$ times the frequency of $\dot{E}_L$, over all $\dot{E}_{L_1}$'s to obtain ΣR_1.
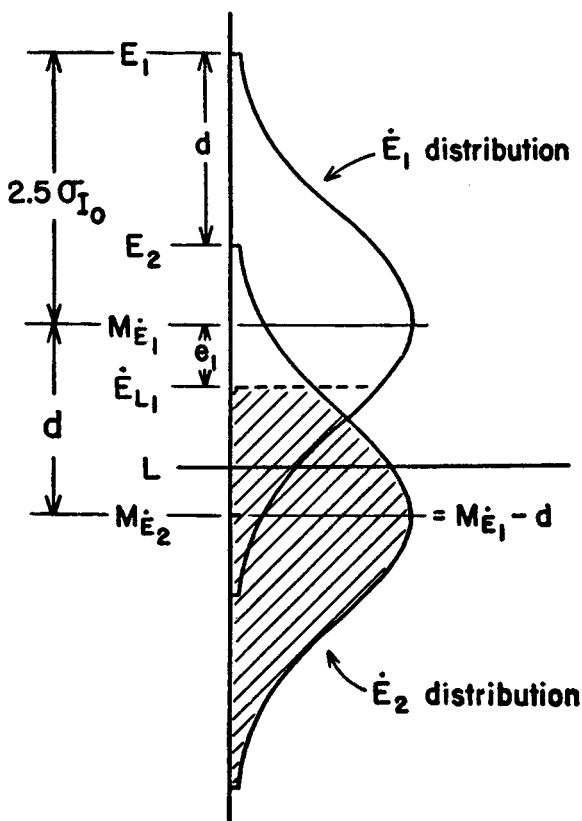
Fig. 79. Graphic representation of the elemental case in the derivation of the probabilities of occurrence of two mutually incompatible responses, R_1 and R_2, as their momentary reaction potential ($\dot{E}$) distributions move across the reaction threshold with a constant difference in reaction potential. Shown above is a typical case in which both $\dot{E}$ distributions are partially subthreshold.

The total number of times that R_2 can happen is obtained in a similar fashion. Considering occurrences of $\dot{E}_{L_2}$'s only, we write by the same logic used in the case of R_1:

$$(4) \quad P\{R_2 \mid \dot{E}_{L_2}\} = f\left[\frac{\dot{E}_{L_2} - M_{\dot{E}_1}}{\sigma_{I_o}}\right]$$
$$= f\left[\dot{E}_{L_2} - (M_{\dot{E}_2} + d)\right]$$
$$= f(e_2 - d),$$

where f again is the normal probability integral, and $e_2 = \dot{E}_{L_2}$ in standard score form. The total frequency of R_2 for any $\dot{E}_{L_2}$ is the product, the area of the normal curve from $-\infty$ to $e_2 - d$ times the ordinate of the normal curve at $x/\sigma = e_2$. The sum of these products over all $\dot{E}_{L_2}$'s is the total frequency of R_2, ΣR_2. We may now compute $R_1\%$ by entering equation (2) with the appropriate values of ΣR_1 and ΣR_2.

Table VI shows the steps involved in the calculation of $R_1\%$ as the $\dot{E}$ distributions move up across the reaction threshold in steps of $.1\sigma$ with $d = .5\sigma$. In column 1 we have specified the various positions of the $\dot{E}_1$ distribution in terms of the height of this distribution above the threshold $(E_1 - L)$, starting from the lowest position in row 1 and moving up as we go down the column. Thus, the first entry in column 1 is $.1\sigma$, as a value $.1\sigma$ smaller would place the $\dot{E}_1$ distribution completely below the threshold, prohibiting the occurrence of either R_1 or R_2 (since E_1 has been assumed to exceed E_2). Note that several row entries have been omitted to save space. In column 2 we have entered the smallest possible superthreshold $\dot{E}_1$ in the $\dot{E}_1$ distribution as located in column 1 by substituting in the equation: $e_1 = 2.5 - (E_1 - L)$. Thus, when $E_1 - L = .1\sigma$ the smallest superthreshold $e_1 = 2.4\sigma$. Column 3 gives the relative frequency of e_1, i.e., the ordinate of the normal curve at e_1. In column 4 we have entered $e_1 + d$ and in column 5 the ordinate of the normal probability integral at $e_1 + d$, or $P\{R_1 \mid \dot{E}_{L_1}\}$, again noting that $\dot{E}_{L_1}$ is the smallest $\dot{E}_{L_1}$ for that position of the $\dot{E}_1$ distribution. Entries in column 6 were calculated by summing the products of the values in columns 3 and 5 over all rows above and including the row of the entry being computed. That is, we have summed $P\{R_1 \mid \dot{E}_{L_1}\}$ times the frequency of $\dot{E}_{L_1}$ for all $\dot{E}_{L_1}$'s at the given location of the $\dot{E}_1$ distribution, which we have

TABLE VI

A table showing the several steps in the calculation of the probability of occurrence of the dominant response (R_1) in two-response selective learning as the momentary reaction potential distributions of the competing response move across the reaction threshold with a constant difference ($d = .5\,\sigma_{r_0}$) in reaction potential.

$$d = .5$$

1	2	3	4	5	6	7	8	9	10	11	12	13
$E_1 - L$	e_1	y_{σ_1}	$e_1 + d$	$P\{R_1 \mid E_{L_1}\}$	ΣR_1	$E_2 - L$	e_2	y_{σ_2}	$e_2 - d$	$P\{R_2 \mid E_{L_2}\}$	ΣR_2	$R_1\%$
.1	2.4	.022	2.9	.998	.023						.000	100
.2	2.3	.028	2.8	.997	.050						.000	100
.3	2.2	.036	2.7	.996	.086						.000	100
.4	2.1	.044	2.6	.995	.130						.000	100
.5	2.0	.054	2.5	.994	.183						.000	100
.6	1.9	.066	2.4	.992	.249	.1	2.4	.022	1.9	.971	.021	92.2
.7	1.8	.079	2.3	.989	.327	.2	2.3	.028	1.8	.964	.048	87.2
.8	1.7	.094	2.2	.986	.419	.3	2.2	.036	1.7	.955	.082	83.6
.9	1.6	.111	2.1	.982	.528	.4	2.1	.044	1.6	.945	.124	81.0
1.0	1.5	.130	2.0	.977	.655	.5	2.0	.054	1.5	.933	.175	78.9
1.1	1.4	.150	1.9	.971	.801	.6	1.9	.066	1.4	.919	.235	77.3
1.2	1.3	.171	1.8	.964	.966	.7	1.8	.079	1.3	.908	.307	75.9
1.3	1.2	.194	1.7	.955	1.151	.8	1.7	.094	1.2	.885	.390	74.7
1.4	1.1	.218	1.6	.945	1.357	.9	1.6	.111	1.1	.864	.486	73.6
1.6	0.9	.266	1.4	.919	1.827	1.1	1.4	.150	0.9	.816	.718	71.8
1.8	0.7	.312	1.2	.885	2.365	1.3	1.2	.194	0.7	.758	.999	70.3
2.0	0.5	.352	1.0	.841	2.949	1.5	1.0	.242	0.5	.692	1.325	69.0
2.2	0.3	.381	0.8	.788	3.550	1.7	0.8	.290	0.3	.618	1.678	67.9
2.4	0.1	.397	0.6	.726	4.134	1.9	0.6	.333	0.1	.540	2.039	67.0
2.6	−0.1	.397	0.4	.655	4.670	2.1	0.4	.368	−0.1	.460	2.384	66.2
2.9	−0.4	.368	0.1	.540	5.331	2.4	0.1	.397	−0.4	.345	2.831	65.3
3.2	−0.7	.312	−0.2	.421	5.792	2.7	−0.2	.391	−0.7	.242	3.158	64.7
3.5	−1.0	.242	−0.5	.309	6.069	3.0	−0.5	.352	−1.0	.159	3.362	64.4
4.0	−1.5	.130	−1.0	.159	6.261	3.5	−1.0	.242	−1.5	.067	3.509	64.1
5.0	−2.5	.018	−2.0	.023	6.309	4.5	−2.0	.054	−2.5	.006	3.546	64.0

shown to be the total number of expected R_1 occurrences (ΣR_1) at that location. For example, when $E_1 - L = 5.0$ the entry in column 6 represents the summed products of the values in columns 3 and 5 over $E_1 - L = .1, .2, .3, \ldots, 4.8, 4.9, 5.0$.

The entries in columns 7–12 represent a corresponding set of calculations for the $\dot{E}_2$ distribution, following the computation of $E_2 - L$ by entering in the equation: $E_2 - L = (E_1 - L) - d$. In other words, a value in column 7 represents the height of the $\dot{E}_2$ distribution above the threshold, given the height of the $\dot{E}_1$ distribution above the threshold. As in the case of the $\dot{E}_1$ distribution, negative or zero values of $E_2 - L$ indicate that the $\dot{E}_2$ distribution is completely below threshold and therefore that R_2 may not occur. Accordingly, zero values of ΣR_2 were entered for below-threshold positions of the $\dot{E}_2$ distribution. Finally, the entries in column 13 were computed by entering equation (2) with appropriate values of ΣR_1 and ΣR_2. The results of these calculations appear graphically in Figures 64 and 71.

: *Derivation of Equation Giving Ordinal Number of Trial on Which Maximal Choice of Two Competing Responses Will Occur*

THE PROBLEM is to calculate the ordinal number of the trial on which maximal choice of R_A will, theoretically, occur when the strengths of two mutually incompatible responses, R_A and R_B, are manipulated by procedures which insure that the number of reinforced occurrences of R_A is always twice that of R_B at any point in training with all other factors held constant. By assumption:

$$(1) \quad R_A\% = f(E_A - E_B)$$

where $R_A\%$ = the percentage choice of R_A, $(E_A - E_B)$ = the differential excitatory potential of the two responses, and f = the normal probability integral. Also, by assumption:

$$(2) \quad E = M(1 - 10^{-iN})$$

where M = the excitatory potential maximum determined by the motivational conditions, i = the rate of approach to the maximum, and N = number of reinforced trials. Since in the situation under consideration the same subjects perform both responses under identical motivational conditions, we may write:

$$(3) \quad E_A = M(1 - 10^{-iNa}), \text{ and}$$

$$(4) \quad E_B = M(1 - 10^{-iNa/2})$$

where Na = the number of reinforced occurrences of R_A, and therefore $Na/2$ = the number of reinforced occurrences of R_B. Subtracting (4) from (3):

$$(5) \quad E_A - E_B = M(1 - 10^{-iNa}) - M(1 - 10^{-iNa/2})$$
$$= M(10^{-iNa/2} - 10^{-iNa}).$$

Substituting (5) in (1):

$$(6) \quad R_A\% = f[M(10^{-iNa/2} - 10^{-iNa})]$$

The trial in which $R_A\%$ is maximal may be located by setting the first derivative of (6) equal to zero as follows:

$$\frac{dR_A\%}{dNa} = M[\ -\ i/2(10^{-iNa/2})\ -\ i(-10^{-iNa})] = 0$$

Solving for Na:

$$i\left[(10^{iNa} - \tfrac{1}{2}(10^{-iNa/2})\right] = 0$$
$$10^{-iNa} = \tfrac{1}{2}(10^{-iNa/2})$$
$$2(10^{-iNa}) = 10^{-iNa/2}$$

Taking logs of both sides:

$$(7)\quad \log_{10}2 - iNa = -iNa/2$$
$$\log_{10}2 = iNa/2$$
$$Na = \frac{2\ \log_{10}2}{i}$$

where $Na =$ the number of reinforced occurrences of R_A preceding the free-choice trial in which maximal choice of R_A is expected.

Equation (7) may be generalized to all ratios of occurrences of R_A and $R_B(A : B)$, by substituting B/A Na for the number of reinforced occurrences of R_B in equation (4). Proceeding as above we obtain:

$$(8)\quad Na = \frac{\log_{10}A - \log_{10}B}{i - B/Ai}$$

Bibliography

AMSEL, A. (1950a) The combination of a primary appetitional need with primary and secondary emotionally derived needs. *J. Exp. Psychol., 40,* 1–14.

AMSEL, A. (1950b) The effect upon level of consummatory response of the addition of anxiety to a motivational complex. *J. Exp. Psychol., 40,* 709–15.

AMSEL, A., and COLE, K. F. (1953) Generalization of fear-motivated interference with water intake. *J. Exp. Psychol., 46,* 243–7.

AMSEL, A., and MALTZMAN, I. (1950) The effect upon generalized drive strength of emotionality as inferred from the level of consummatory response. *J. Exp. Psychol., 40,* 563–9.

BAKAN, D. (1953) Learning and the scientific enterprise. *Psychol. Rev., 60,* 45–9.

BAKAN, D. (1954) A generalization of Sidman's results on group and individual functions, and a criterion. *Psychol. Bull., 51,* 63–4.

BEKHTEREV, V. M. (1909) Die objektive untersuchung der nevro-psychischen Sphare der Geisteskranken. *2. Psychother. med. Psychol., 1,* 257–89.

BEKHTEREV, V. M. (1913) *Objektive Psychologie oder Psychore-flexologie. Die Lehre von den Assoziationsreflexen.* Leipzig, Teubner.

BERGMANN, G. (1953) Theoretical psychology. In Stone, C. P., ed., *Annual review of psychology.* Stanford, Annual Reviews, *4,* 435–58.

BERGMANN, G. (1954) *The metaphysics of logical positivism.* New York, Longmans, Green.

BERGMANN, G. (1954) Sense and nonsense in operationism. *Sci. Mon., 79,* 210–4.

BERGMANN, G., and SPENCE, K. W. (1941) Operationism and theory in psychology. *Psychol. Rev., 48,* 1–14.

BLOOMBERG, R., and WEBB, W. B. (1949) Various degrees within a single drive as cues for spacial response learning in the white rat. *J. Exp. Psychol., 39,* 628–36.

BORING, E. G. (1933) *The physical dimensions of consciousness.* New York, Appleton-Century.

BRANDAUER, C. M. (1953) A confirmation of Webb's data concerning the action of irrelevant drives. *J. Exp. Psychol., 45,* 150-2.

BRIDGMAN, P. W. (1927) *The logic of modern physics.* New York, Macmillan.

BROGDEN, W. J. (1949) Acquisition and extinction of a conditioned avoidance response in dogs. *J. Comp. Physiol. Psychol., 42,* 296–302.

BROGDEN, W. J., LIPMAN, E. A., and CULLER, E. (1938) The role of incentive in conditioning and extinction. *Amer. J. Psychol., 51,* 109–17.

BROWN, J. S., KALISH, H. I., and FARBER, I. E. (1951) Conditioned fear as revealed by magnitude of startle response to an auditory stimulus. *J. Exp. Psychol., 41,* 317–28.

BURKE, C. J. (1948) The function relating momentary effective reaction potential to running-time in a straight runway. (Unpublished doctor's dissertation, State University of Iowa.)

CAMPBELL, B. A., and KRAELING, DORIS (1953) Response strength as a function of drive level and amount of drive reduction. *J. Exp. Psychol., 45,* 97–101.

CAMPBELL, B. A., and KRAELING, DORIS (1954) Response strength as a function of drive level during training and extinction. *J. Comp. Physiol. Psychol., 47,* 101–3.

CARLTON, P. L. (1954) Response strength as a function of delay of reward and physical confinement. (Unpublished master's dissertation, State University of Iowa.)

CARNAP, R. (1936, 1937) Testability and meaning. *Philos. Sci., 3,* 419–71; *4,* 1–40.

CHILD, I. L. (1954) Personality. *Annu. Rev. Psychol., 5,* 149–70.

COTTON, J. W. (1953) Running time as a function of amount of food deprivation. *J. Exp. Psychol., 46,* 188–98.

COWLES, J. T., and NISSEN, H. W. (1937) Reward-expectancy in delayed responses of chimpanzees. *J. Comp. Psychol., 24,* 345–58.

CRESPI, L. P. (1942) Quantitative variation of incentive and performance in the white rat. *Amer. J. Psychol., 55,* 467–517.

CRESPI, L. P. (1944) Amount of reinforcement and level of performance. *Psychol. Rev., 51,* 341–57.

CZEH, R. S. (1954) Response strength as a function of the magnitude of the incentive and consummatory time in the goal box. (Unpublished master's dissertation, State University of Iowa.)

ESTES, W. K. (1950) Toward a statistical theory of learning. *Psychol. Rev., 57,* 94–107.

ESTES, W. K., and BURKE, C. J. (1953) A theory of stimulus variability in learning. *Psychol. Rev., 60,* 276–86.

ESTES, W. K., KOCH, S., MacCORQUODALE, K., MEEHL, P. E., MUELLER,

C. G., SCHOENFELD, W. N., and VERPLANK, W. S. (1954) *Modern learning theory*. New York, Appleton-Century-Crofts.

ESTES, W. K., and SKINNER, B. F. (1941) Some quantitative properties of anxiety. *J. Exp. Psychol.*, *29*, 390–400.

FARBER, I. E., and SPENCE, K. W. (1953) Complex learning and conditioning as a function of anxiety. *J. Exp. Psychol.*, *45*, 120–5.

FESTINGER, L. (1943) Development of differential appetite in the rat. *J. Exp. Psychol.*, *32*, 226–34.

FINAN, J. L. (1940) Quantitative studies in motivation: I. Strength of conditioning in rats under varying degrees of hunger. *J. Comp. Psychol.*, *29*, 119–34.

FINCH, G. (1938) Hunger as a determinant of conditional and unconditional salivary response magnitude. *Amer. J. Physiol.*, *123*, 379–82.

GRICE, G. R. (1942) An experimental study of the gradient of reinforcement in maze learning. *J. Exp. Psychol.*, *30*, 475–89.

GUTHRIE, E. R. (1930) Conditioning as a principle of learning. *Psychol. Rev.*, *37*, 412–28.

GUTHRIE, E. R. (1935) *The psychology of learning*. New York, Harper.

GUTTMAN, N. (1953) Operant conditioning, extinction, and periodic reinforcement in relation to concentration of sucrose used as reinforcing agent. *J. Exp. Psychol.*, *46*, 213–24.

GUTTMAN, N. (1954) Equal-reinforcement values for sucrose and glucose solutions compared with equal sweetness values. *J. Comp. Physiol. Psychol.*, *47*, 358–63.

HAAGEN, C. H. (1949) Synonymity, vividness, familiarity, and association value ratings of 400 pairs of common adjectives. *J. Psychol.*, *27*, 453–63.

HARKER, G. S. (1950) An experimental investigation of the effects of changes in the delay of reinforcement upon level of performance of an instrumental response. (Unpublished doctor's dissertation, State University of Iowa.)

HARLOW, H. F. (1953) Mice, monkeys, men and motives. *Psychol. Rev.*, *60*, 23–32.

HEMPEL, C. G. (1952) *Fundamentals of concept formation in empirical science*. Chicago, University of Chicago Press.

HERON, W. T., and SKINNER, B. F. (1937) Changes in hunger during starvation. *Psychol. Rec.*, *1*, 51–60.

HILGARD, E. R. (1931) Conditioned eyelid reactions to a light stimulus based on the reflex wink to sound. *Psychol. Monogr.*, *41*, No. 184.

HILGARD, E. R. (1938) A summary and evaluation of alternative

procedures for the construction of Vincent curves. *Psychol. Bull.,* *35,* 282–97.

HILGARD, E. R. (1948) *Theories of learning.* New York, Appleton-Century-Crofts.

HILGARD, E. R., and CAMPBELL, A. A. (1936) The course of acquisition and retention of conditioned eyelid responses in man. *J. Exp. Psychol., 21,* 310–9.

HILGARD, E. R., JONES, L. U., and KEPLAN, S. J. (1951) Conditioned discrimination as related to anxiety. *J. Exp. Psychol., 42,* 94–9.

HILGARD, E. R., and MARQUIS, D. G. (1935) Acquisition, extinction, and retention of conditioned lid responses to light in dogs. *J. Comp. Psychol., 19,* 29–58.

HILGARD, E. R., and MARQUIS, D. G. (1936) Conditioned eyelid responses in monkeys, with a comparison of dog, monkey, and man. *Psychol. Monogr., 47,* No. 212, 186–98.

HORENSTEIN, BETTY R. (1951) Performance of conditioned responses as a function of strength of hunger drive. *J. Comp. Physiol. Psychol., 44,* 210–24.

HOVLAND, C. I. (1937) The generalization of conditioned responses: IV. The effects of varying amounts of reinforcement upon the degree of generalization of conditioned responses. *J. Exp. Psychol., 21,* 261–76.

HULL, C. L. (1929) A functional interpretation of the conditioned reflex. *Psychol. Rev., 36,* 498–511.

HULL, C. L. (1930a) Knowledge and purpose as habit mechanisms. *Psychol. Rev., 37,* 511–25.

HULL, C. L. (1930b) Simple trial and error learning: A study in psychological theory. *Psychol. Rev., 37,* 241–56.

HULL, C. L. (1931) Goal attraction and directing ideas conceived as habit phenomena. *Psychol. Rev., 38,* 487–506.

HULL, C. L. (1932) The goal gradient hypothesis and maze learning. *Psychol. Rev., 39,* 25–43.

HULL, C. L. (1934a) The concept of the habit-family hierarchy and maze learning. *Psychol. Rev., 41,* 33–54.

HULL, C. L. (1934b) The rat's speed-of-locomotion gradient in the approach to food. *J. Comp. Psychol., 17,* 393–422.

HULL, C. L. (1934c) Learning: II. The factor of the conditioned reflex. In Murchison, C., ed., *A handbook of general experimental psychology.* Worcester, Mass., Clark University Press. Pp. 382–455.

HULL, C. L. (1935) Special review: Thorndike's *Fundamentals of learning. Psychol. Bull., 32,* 807–23.

HULL, C. L. (1937) Mind, mechanism, and adaptive behavior. *Psychol. Rev., 44,* 1–32.

Hull, C. L. (1943) *Principles of behavior.* New York, Appleton-Century-Crofts.

Hull, C. L. (1950) Behavior postulates and corollaries.—1949. *Psychol. Rev., 57,* 173–80.

Hull, C. L. (1951) *Essentials of behavior.* New Haven, Yale University Press.

Hull, C. L. (1952) *A behavior system.* New Haven, Yale University Press.

Hull, C. L., Hovland, C. I., Ross, R. T., Hall, M., Perkins, D. T., and Fitch, F. B. (1940) *Mathematico-deductive theory of rote learning: A study in scientific methodology.* New Haven, Yale University Press.

Humphreys, L. G. (1940) Distributed practice in the development of the conditioned eyelid reaction. *J. Gen. Psychol., 22,* 379–85.

Humphreys, L. G. (1943) Measures of strength of conditioned eyelid responses. *J. Gen. Psychol., 29,* 101–11.

Hunter, W. S. (1935) Conditioning and extinction in the rat. *Brit. J. Psychol., 26,* 135–48.

James, W. (1892) *Text-book of psychology: Briefer course.* New York, Holt.

Jenkins, J. J., and Hanratty, Jacqueline A. (1949) Drive intensity discrimination in the albino rat. *J. Comp. Physiol. Psychol., 42,* 228–32.

Kalish, H. I. (1954) Strength of fear as a function of the number of acquisition and extinction trials. *J. Exp. Psychol., 47,* 1–9.

Kaplan, M. (1952) The effects of noxious stimulus intensity and duration during intermittent reinforcement of escape behavior. *J. Comp. Physiol. Psychol., 45,* 538–49.

Katz, D. (1937) *Animals and men.* London, Longmans, Green.

Keller, F. S. (1941) Light aversion in the white rat. *Psychol. Rec., 4,* 235–50.

Keller, F. S., and Schoenfeld, W. N. (1950) *Principles of psychology.* New York, Appleton-Century-Crofts.

Kelly, T. L. (1924) *Statistical method.* New York, Macmillan.

Kendler, H. H. (1945a) Drive interaction: I. Learning as a function of the simultaneous presence of the hunger and thirst drives. *J. Exp. Psychol., 35,* 96–109.

Kendler, H. H. (1945b) Drive interaction: II. Experimental analysis of the role of drive in learning theory. *J. Exp. Psychol., 35,* 188–98.

Kendler, H. H. (1946) The influence of simultaneous hunger and thirst drives upon the learning of two opposed spacial responses of the white rat. *J. Exp. Psychol., 36,* 212–20.

KENDLER, H. H. (1947) An investigation of latent learning in a T-maze. *J. Comp. Physiol. Psychol., 40,* 265–70.

KENT, GRACE H., and ROSANOFF, A. J. (1910) A study of association in insanity. *Amer. J. Insan., 67,* 37–96, 317–90.

KESSEN, W. (1953) Response strength and conditioned stimulus intensity. *J. Exp. Psychol., 45,* 82–6.

KETCHEL, RHODA (1955) Performance in instrumental learning as a function of shock intensity. (Unpublished master's dissertation, State University of Iowa.)

KIMBLE, G. A. (1951) Behavior strength as a function of the intensity of the hunger drive. *J. Exp. Psychol., 41,* 341–8.

KLEITMAN, N., and CRISLER, G. A. (1927) A quantitative study of a salivary conditioned reflex. *Amer. J. Physiol., 79,* 571–614.

KOCH, S., and DANIEL, W. J. (1945) The effect of satiation on the behavior mediated by a habit of maximum strength. *J. Exp. Psychol., 35,* 167–87.

LEWIN, K. (1935) *Principles of topological psychology.* New York, McGraw-Hill.

LOESS, H. B. (1952) The effect of variation of motivational level and changes in motivational level on performance in learning. (Unpublished doctor's dissertation, State University of Iowa.)

LOGAN, F. A. (1952) The role of delay of reinforcement in determining reaction potential. *J. Exp. Psychol., 43,* 393–9.

LUCHINS, A. S., and LUCHINS, EDITH H. (1954) Variables and functions. *Psychol. Rev., 61,* 315–22.

MCALLISTER, W. R. (1953) The effect on eyelid conditioning of shifting the CS-US interval. *J. Exp. Psychol., 45,* 423–8.

MACDONALD, ANNETTE (1946) The effect of adaptation to the unconditioned stimulus upon the formation of conditioned avoidance response. *J. Exp. Psychol., 36,* 1–12.

MELTON, A. W. (1936) The methodology of experimental studies of human learning and retention: I. The functions of a methodology and the available criteria for evaluating different experimental methods. *Psychol. Bull., 33,* 305–94.

MERRELL, MARGARET (1931) The relationship of individual growth to average growth. *Hum. Biol., 3,* 37–70.

MILLER, N. E. (1948) Studies of fear as an acquirable drive: I. Fear as motivation and fear-reduction as reinforcement in the learning of new responses. *J. Exp. Psychol., 38,* 89–101.

MILLER, N. E. (1951) Learnable drives and rewards. In Stevens, S. S., ed., *Handbook of experimental psychology.* New York, Wiley. Pp. 435–72.

MILLER, N. E., and DOLLARD, J. (1941) *Social learning and motiva-tion.* New Haven, Yale University Press.

MONTAGUE, E. K. (1953) The role of anxiety in serial rote learning. *J. Exp. Psychol., 45,* 91–6.

MORGAN, C. L. (1896) *Introduction to comparative psychology.* London, Scott.

MORGAN, C. T. (1943) *Physiological psychology.* New York, McGraw-Hill.

MOWRER, O. H. (1947) On the dual nature of learning—a re-interpretation of "conditioning" and "problem-solving." *Harvard Educ. Rev., 17,* 102–48.

MURFIN, F. L. (1954) The relationship of fear to the CS-UCS interval in acquisition and the CS-duration in extinction. (Unpublished doctor's dissertation, State University of Iowa.)

MURRAY, H. A. (1938) *Explorations in personality.* New York, Oxford University Press.

MYERS, A. K., and MILLER, N. E. (1954) Failure to find a learned drive based on hunger; evidence for learning motivated by "exploration." *J. Comp. Physiol. Psychol., 47,* 428–36.

NAGATY, M. O. (1951) The effect of reinforcement on closely following S-R connections: I. The effect of a backward conditioning procedure on the extinction of conditioned avoidance. *J. Exp. Psychol., 42,* 239–46.

NISSEN, H. W., and ELDER, J. H. (1935) The influence of amount of incentive on delayed response performances of chimpanzees. *J. Genet. Psychol., 47,* 49–72.

PASSEY, G. E. (1948) The influence of intensity of unconditional stimulus upon acquisition of a conditional response. *J. Exp. Psychol., 38,* 420–8.

PAVLOV, I. P. (1906) The scientific investigation of the psychical faculties or processes in the higher animals. *Science, 24,* 613–9.

PAVLOV, I. P. (1927) *Conditioned reflexes.* G. V. Anrep, trans. London, Oxford University Press.

PERIN, C. T. (1942) Behavior potentiality as a joint function of the amount of training and the degree of hunger at the time of extinction. *J. Exp. Psychol., 30,* 93–113.

PERIN, C. T. (1943) A quantitative investigation of the delay-of-reinforcement gradient. *J. Exp. Psychol., 32,* 37–51.

RAMOND, C. K. (1953) Anxiety and task as determiners of verbal performance. *J. Exp. Psychol., 46,* 120–4.

RAMOND, C. K. (1954a) Performance in instrumental learning as a

joint function of delay of reinforcement and time of deprivation. *J. Exp. Psychol., 47,* 248–50.

RAMOND, C. K. (1954b) Performance in selective learning as a function of hunger. *J. Exp. Psychol., 48,* 265–70.

RASHEVSKY, N. (1938) *Mathematical biophysics.* Chicago, University of Chicago Press.

SALTZMAN, I., and KOCH, S. (1948) The effect of low intensities of hunger on the behavior mediated by a habit of maximum strength. *J. Exp. Psychol., 38,* 347–70.

SCHLOSBERG, H. (1937) The relationship between success and the laws of conditioning. *Psychol. Rev., 44,* 379–99.

SHEFFIELD, F. D. (1948) Avoidance training and the contiguity principle. *J. Comp. Physiol. Psychol., 41,* 165–77.

SHEFFIELD, F. D., and ROBY, T. B. (1950) Reward value of a non-nutritive sweet taste. *J. Comp. Physiol. Psychol., 43,* 471–81.

SHEFFIELD, F. D., ROBY, T. B., and CAMPBELL, B. A. (1954) Drive reduction versus consummatory behavior as determinants of reinforcement. *J. Comp. Physiol. Psychol., 47,* 349–54.

SHEFFIELD, F. D., WULFF, J. J., and BACKER, R. (1951) Reward value of copulation without sex drive reduction. *J. Comp. Physiol. Psychol., 44,* 3–8.

SHILLING, M. (1951) An experimental investigation of the effect of a decrease in the delay of reinforcement upon instrumental response performance. (Unpublished master's dissertion, State University of Iowa.)

SIDMAN, M. (1952) A note on functional relations obtained from group data. *Psychol. Bull., 49,* 263–9.

SIEGEL, P. S. (1946) Alien drive, habit strength, and resistance to extinction. *J. Comp. Psychol., 39,* 307–17.

SIEGEL, P. S., and BRANTLEY, J. J. (1951) The relationship of emotionality to the consummatory response of eating. *J. Exp. Psychol., 42,* 304–6.

SIEGEL, P. S., and SIEGEL, HELEN S. (1949) The effect of emotionality on the water intake of the rat. *J. Comp. Physiol. Psychol., 42,* 12–6.

SKINNER, B. F. (1935) Two types of conditioned reflex and a pseudo type. *J. Gen. Psychol., 12,* 66–77.

SKINNER, B. F. (1938) *The behavior of organisms.* New York, Appleton-Century-Crofts.

SPENCE, K. W. (1936) The nature of discrimination learning in animals. *Psychol. Rev., 43,* 427–49.

SPENCE, K. W. (1937) The differential response in animals to stimuli varying within a single dimension. *Psychol. Rev., 44,* 430–44.

SPENCE, K. W. (1941) Symposium: Learning as related to need and

the subsequent motivation of such learned behavior. *Psychol. Bull., 38,* 721.

Spence, K. W. (1944) The nature of theory construction in psychology. *Psychol. Rev., 51,* 47–68.

Spence, K. W. (1947) The role of secondary reinforcement in delayed reward learning. *Psychol. Rev., 54,* 1–8.

Spence, K. W. (1948) The postulates and methods of behaviorism. *Psychol. Rev., 55,* 67–78.

Spence, K. W. (1951a) Theoretical interpretations of learning. In Stone, C. P., ed., *Comparative psychology.* 3d ed. New York, Prentice-Hall.

Spence, K. W. (1951b) Theoretical interpretations of learning. In Stevens, S. S., ed., *Handbook of experimental psychology.* New York, Wiley.

Spence, K. W. (1952) Mathematical formulations of learning phenomena. *Psychol. Rev., 59,* 152–60.

Spence, K. W. (1953a) Learning and performance in eyelid conditioning as a function of the intensity of the UCS. *J. Exp. Psychol., 45,* 57–63.

Spence, K. W. (1953b) Mathematical theories of learning. *J. Gen. Psychol., 49,* 283–91.

Spence, K. W. (1954) The relation of response latency and speed to the intervening variables and N in S-R theory. *Psychol. Rev., 61,* 209–16.

Spence, K. W., and Beecroft, R. S. (1954) Differential conditioning and level of anxiety. *J. Exp. Psychol., 48,* 399–403.

Spence, K. W., Bergmann, G., and Lippitt, R. (1950) A study of simple learning under irrelevant motivational-reward conditions. *J. Exp. Psychol., 40,* 539–51.

Spence, K. W., and Farber, I. E. (1953) Conditioning and extinction as a function of anxiety. *J. Exp. Psychol., 45,* 116–9.

Spence, K. W., and Lippitt, R. (1940) "Latent" learning of a simple maze problem with relevant needs satiated. *Psychol. Bull., 37,* 429.

Spence, K. W., and Lippitt, R. (1946) An experimental test of the sign-gestalt theory of trial and error learning. *J. Exp. Psychol., 36,* 491–502.

Spence, K. W., and Taylor, Janet (1951) Anxiety and strength of the UCS as determiners of the amount of eyelid conditioning. *J. Exp. Psychol., 42,* 183–8.

Stein, L. (1953) Performance in selective learning as a function of delay of reinforcement. (Unpublished master's thesis, State University of Iowa.)

Stein, L. (1955) Unpublished experiment.

STELLAR, E. (1954) The physiology of motivation. *Psychol. Rev.*, *61*, 5–22.

STEVENS, S. S. (1939) Psychology and the science of science. *Psychol. Bull.*, *36*, 221–63.

STRASSBURGER, R. C. (1950) Resistance to extinction of a conditional operant as related to drive level of reinforcement. *J. Exp. Psychol.*, *40*, 473–87.

SWISHER, D. E. (1951) The role of magnitude of reward in determining instrumental performance level. (Unpublished doctor's dissertation, State University of Iowa.)

TAYLOR, JANET A. (1951) The relationship of anxiety to the conditioned eyelid response. *J. Exp. Psychol.*, *41*, 81–92.

TAYLOR, JANET A., and SPENCE, K. W. (1952) The relationship of anxiety level to performance in serial learning. *J. Exp. Psychol.*, *44*, 61–4.

TEEL, K. S. (1952) Habit strength as a function of motivation during learning. *J. Comp. Physiol. Psychol.*, *45*, 188–91.

TEEL, K. S., and WEBB, W. B. (1951) Response evocation on satiated trials in the T-maze. *J. Exp. Psychol.*, *41*, 148–52.

THORNDIKE, E. L. (1898) Animal intelligence. An experimental study of the associative processes in animals. *Psychol. Monogr.*, *2*, No. 8.

THORNDIKE, E. L. (1932) *The fundamentals of learning.* New York, Teachers College.

THORNDIKE, E. L. (1935) *The psychology of wants, interests, and attitudes.* New York, Appleton-Century.

TOLMAN, E. C. (1932) *Purposive behavior in animals and men.* New York, Century.

TOLMAN, E. C. (1934) Theories of learning. In Moss, F. A., ed., *Comparative psychology.* New York, Prentice-Hall. Pp. 367–408.

TOLMAN, E. C. (1949) There is more than one kind of learning. *Psychol. Rev.*, *56*, 144–55.

TOLMAN, E. C., and GLEITMAN, H. (1949) Studies in spatial learning: VII. Place and response learning under different degrees of motivation. *J. Exp. Psychol.*, *39*, 653–9.

VERPLANCK, W. S., and HAYES, J. R. (1953) Eating and drinking as a function of maintenance schedule. *J. Comp. Physiol. Psychol.*, *46*, 327–33.

VINCENT, STELLA B. (1912) The function of the vibrissae in the behavior of the white rat. *Behav. Monogr.*, *1*, No. 5.

VOEKS, VIRGINIA W. (1954) Acquisition of S-R connections: A test of Hull's and Guthrie's theories. *J. Exp. Psychol.*, *47*, 137–47.

WARDEN, C. J. (1931) *Animal motivation: Experimental studies on the albino rat.* New York, Columbia University Press.

WATSON, J. B. (1913) Psychology as the behaviorist views it. *Psychol. Rev., 20,* 158–77.

WATSON, J. B. (1916) The place of the conditioned-reflex in psychology. *Psychol. Rev., 23,* 89–116.

WATERMAN, A. T., WARD, J. C., JR., and KELLY, M. J. (1954) Scientific research and national security. *Sci. Mon., 78,* 214–24.

WEBB, W. B. (1949) The motivational aspect of an irrelevant drive in the behavior of the white rat. *J. Exp. Psychol., 39,* 1–14.

WILLIAMS, S. B. (1938) Resistance to extinction as a function of the number of reinforcements. *J. Exp. Psychol., 23,* 506–22.

YAMAGUCHI, H. G. (1951) Drive (D) as a function of hours of hunger (h). *J. Exp. Psychol., 42,* 108–17.

YAMAGUCHI, H. G., HULL, C. L., FELSINGER, J. M., and GLADSTONE, A. I. (1948) Characteristics of dispersions based on the pooled momentary reaction potentials $(s\dot{E}R)$ of a group. *Psychol. Rev., 55,* 216–38.

ZEAMAN, D. (1949) Response latency as a function of the amount of reinforcement. *J. Exp. Psychol., 39,* 466–83.

ZENER, K., and MCCURDY, H. G. (1939) Analysis of motivational factors in conditioned behavior: I. The differential effect of changes in hunger upon conditioned, unconditioned, and spontaneous salivary secretion. *J. Psychol., 8,* 321–50.

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